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Empirical Essays on the Economic Analysis of Social Connections

by

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Declaration of Authorship

I, Tommaso Colussi, declare that this thesis titled, "Empirical Essays on the Economic Analysis of Social Connections", and the work presented in it are my own. I confirm that:

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- Where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated.
- Where I have consulted the published work of others, this is always clearly attributed.
- Where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work.
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Abstract

Social connections represent an important determinant of economic agents' behaviour. The three chapters of this thesis empirically analyse the effect of different types of networks on several economic outcomes.

The first chapter analyses the role played by co-worker networks on immigrants' employment outcomes. It investigates how immigrants' job search outcomes are affected by the labour market outcomes of co-workers from the same country of origin. Using matched employer-employee micro data from Italy and an instrumental variables approach, I show that an increase in the employment prospects of socially connected workers improves immigrants' job search outcomes. The paper also sheds light on the different mechanisms generating the social effect and it highlights the role of migrant networks in explaining immigrant segregation.

Chapter 2 employs a unique dataset on articles, authors and editors of the top four economics journals over the period 2000-2006 to investigate the role of social ties in the publication process. Connections between editors and authors are identified based on their academic histories. Regression results show that the existence of a social tie with an editor positively affects publication outcomes of connected scholars. The analysis of citations shows that *connected* articles receive on average a higher number of citations than *non-connected* ones.

The final chapter focuses on the impact of female managers on female workers' employment outcomes. Exploiting changes in the share of female managers induced by firms' takeovers, I find no statistically significant effect of an increase in the presence of female managers on employment outcomes of female workers. However there is an interesting negative effect on wage inequality within the acquiring firm, which may matter for both equity and efficiency reasons.

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Chapter 1

Migrant Networks and Job Search Outcomes: Evidence from Displaced Workers

1.1 Introduction

This paper uses social security micro data from Italy spanning over more than twenty-five years on the universe of private sector employees in order to analyze how migrants' networks affect job search outcomes of their displaced members.

Workers, either employed or unemployed, often use their personal contacts to acquire information about job vacancies (Ioannides and Loury 2004); similarly, firms tend to rely on employee referrals as they reduce information uncertainties when screening new job applicants (Dustman, Glitz and Schoenberg 2010). Figure 1.1 plots the share of private sector employees who received information about their current job through their acquaintances across a number of European countries.[1]

[1] Data come from The European Community Household Panel, which is a longitudinal dataset covering 15 countries of the European Union for the period 1994-2001. Several countries, like Luxembourg, Sweden, Finland, Austria and Denmark are excluded from the sample as they are not covered in all the waves. The precise question asked in this survey is: *"By what means were you first informed about your current job?"*. Respondents then have six mutually exclusive alternatives, which include *"Friends, family or personal contacts"*.

On average more than one third of the workers in Europe report that they have obtained their current job through *informal channels*, i.e. through friends or relatives; this share becomes higher in Mediterranean countries where labour markets function imperfectly and hence non-market institutions become relevant (Pellizzari 2010). The share of immigrants relying on their acquaintances while looking for a job is even higher: in Italy for instance, about 42% of immigrants found their current job through personal contacts, compared to a figure for natives of 31%.

Social networks play a key role for immigrant job seekers for several reasons. First, as migrants are often *newcomers* in the labour market, personal contacts help them overcome information asymmetries generally affecting unexperienced workers. Second, members of minority communities are more cohesive and they are more likely to help other members of the same community. In addition, immigrants may systematically rely on personal contacts while unemployed as many of them come from low-income countries where social networks are one of the major sources of job information and support (Munshi 2003).

Many studies find that non-native individuals tend to interact mainly with individuals of the same ethnicity (Bandiera, Barankay and Rasul 2008; Bertrand, Luttmer and Mullainathan 2000; Marmaros and Sacerdote 2006) and that recent immigrants typically locate where earlier immigrants from the same sending country live and work, giving rise to ethnic clusters (Card 2009). Individuals from the same country of origin provide valuable information and support, in turn possibly leading to positive labour market outcomes. In particular, employed network members might provide information on job openings (Calvo-Armengól and Jackson 2004) or directly refer workers to their employers (Montgomery 1991; Dustman, Glitz and Schoenberg 2010), eventually increasing the arrival rate of job offers (Goel and Lang 2010).

A higher employment rate among network members though might also have the opposite effect, as greater network support could reduce job search effort, resulting in longer unemployment duration. General equilibrium effects might also be at work, due to competition in the labour market, possibly offsetting the potential benefits stemming from clustering (Beaman 2011). Ultimately, segregation might reduce the pace of integration and lead to poor labour market outcomes, as it may lower the speed at which immigrants learn host country skills and language or reduce the incentives to relocate to areas where labour demand is stronger (Lazear 1999; Edin, Fredriksson and Aslund 2003; Boeri, De Philippis, Patacchini and Pellizzari 2011).

Whether overall an increase in the employment prospects of socially connected individuals improves or harms job search outcomes among the unemployed remains an open question. This work empirically addresses this issue by focusing on immigrant networks and estimating the effect of changes in the current employment rate of past co-workers from the same country of origin on unemployed individuals' job search outcomes.

For this exercise I use matched employer-employee micro data from the administrative records of the Italian Social Security Administration (INPS) for the Italian region of Veneto (also used in Card, Devicienti and Maida 2011), which cover the universe of private non-agricultural dependent employment relationships between January 1975 and December 2001.

Identifying the effect of social networks on workers' job search outcomes though is not a straightforward empirical exercise. First, because of task and job specialisation along country of origin lines and because of geographical clustering, migrants from the same country tend to be exposed to similar labour demand shocks, a classic case of correlated effects (Moffitt 2001). A positive correlation between a worker's employment status and the employment rate of his co-workers may be driven for example by shocks affecting only specific groups in the same occupation or working in the same local labour market. Second, migrants who tend to cluster with employed individuals might be systematically different; for example, being the ones most benefiting from group membership, a classic case of endogenous group formation, possibly leading to biased estimates of social effects. Finally, reflection plagues any credible attempt to identify social effects (Manski 1993; Moffitt 2001; Soetevent 2006).

In order to take into account these sources of endogeneity, I focus on displaced workers as their decision to work is arguably exogenous. For each of these workers I define a *network* as the group of past co-workers from the same country of origin in the five years preceding the displacement. To solve potential endogeneity issues, I instrument each network member's employment status by his own displacement episode up to the month before the pivotal worker's displacement episode.

A well-established body of literature shows that job loss episodes have long-lasting consequences on employment (von Wachter and Bender 2007). As long as past displacements are uncorrelated with a worker's characteristics, both those that affect or are correlated with socially connected individuals' latent employment outcomes and those affecting the propensity to form a group, this instrumental variable approach will lead to consistent estimates of the effect of interest.

The empirical analysis shows that, among immigrants who lost the job, a 10 percentage point increase in the current employment rate of previous co-workers from the same country of origin raises the probability of re-employment within 36 months by 5.7 percentage points. Separate regressions for low skilled and unexperienced immigrants show that these categories of workers gain the most from the support of past co-workers. The social effect is particularly relevant for immigrants coming from non-OECD countries, where formal labour markets are less developed and where non-market institutions are likely to be prevalent. Further, the magnitude of the social effect increases after the second year following the lay-off: networks appear to constitute an important resort particularly for immigrants with limited access to employment opportunities (Datcher Loury 2006)

Interestingly, I find no evidence of any effect of changes in the employment rate of past co-workers from countries of origin other than the workers' own. Moreover results show that even among natives there is a positive effect of the network employment rate, however this effect is significantly smaller than the one found for immigrants, suggesting that migrants tend to rely more on their acquaintances in job search than natives.

The analysis of post-displacement outcomes sheds light on the different mechanisms behind the estimated network effect. I show that when the network employment rate increases by 10 percentage points, the probability that displaced migrants find a job within 36 months since job loss in *connected* cities and firms, i.e. firms or cities in which at least one past co-worker has ever worked, increases by 7.9 and 5.1 percentage points respectively. These last findings are consistent with the interpretation that migrant networks facilitate the job search of displaced members by providing them with information about job vacancies.

Finally, I find a positive correlation between the degree of workplace segregation, measured by the dissimilarity and isolation indexes, and the magnitude of the social effect across different countries of origin: immigrants who benefit more from the employment status of their co-workers are also the ones who experience relatively higher levels of segregation in the labour market.

The rest of the paper is structured as follows: Section 1.2 describes the data and it provides summary statistics. Section 1.3 discusses the research design and the identification issues. Section 1.4 reports the main results and a set of robustness checks. Section 1.5 analyses post displacement outcomes of displaced migrant workers. Finally, Section 1.6 concludes.

1.2 Data and Summary Statistics

The data used in this paper are matched employer-employee micro data from the administrative records of the Italian Social Security Administration (INPS) for the Italian region of Veneto. The data cover the universe of private non-agricultural dependent employment relationships between January 1975 and December 2001.[2]

[2]Although the data primarily include private sector workers, they also contain information on public sector workers who have fixed term contracts, such as substitute teachers, health professionals and nurses.

This dataset has been used by a number of other papers; among others, Card, Devicienti and Maida (2011) test the degree of rent sharing by workers in Italy.[3]

Veneto is one of the twenty-one Italian regions (administrative divisions corresponding roughly speaking to USA states) encompassing seven provinces (roughly a USA county) and 581 towns.[4] As of 2011, Veneto had a population of about 4.9 million, accounting for about 8% of the total Italian population and 9% of national GDP.[5]

The primary unit of observation in the data is a firm-worker match per calendar year. In other terms, for each employment relationship, there are as many observations in the data as the number of calendar years over which this relationship spans. In each calendar year, there can be multiple observations by individual, as individuals can hold more than one job, whether simultaneously or sequentially, during the same year. The data provide information about start and end dates of any employment relationships, the total yearly compensation, the number of working weeks, the type of contract (part-time vs. full time), worker's occupation, age, gender, and municipality of residence at the time of the first job in Veneto, sector of activity (at the 3 digit level) and the municipality where the firm is located.[6]

[3] Using a similar version of this dataset that encompasses only two provinces, Cingano and Rosolia (2011) assess the strength of information spillovers of past co-workers' employment status on unemployment duration of displaced workers.

[4] This dataset contains 7675 municipalities as workers originally observed in Veneto may be subsequently employed in any Italian municipalities outside Veneto. As of 2011, in Italy there were about 8,200 municipalities.

[5] Veneto is located in the north east of the Italy, the major municipalities, in terms of population, are Venice (270,000 inhabitants), Verona (263,000 inhabitants) and Padua (214,000 inhabitants). The most industrialised cities are Verona, Vicenza, Padua, Treviso, characterised by small firms, operating in different areas of manufacturing: food products, wood and furniture, leather and footwear, textiles and clothing, gold jewellery. Venice and Rovigo are instead specialised in energy, chemical and metal processing. Tourism also plays an important role in the region's economy: Veneto is the first region in Italy in terms of tourist presence, accounting for one-fifth of Italy's foreign tourism. Tattara and Anastasia (2003) provide a report on Veneto's economy.

[6] The dataset is composed of three archives: a *"worker"* archive in which all the time invariant characteristics of the workers are included, such as the date and the country of birth, the gender and the municipality of residence at the time he started to work in Veneto; a *"job"* archive, in which information on the employment relationships is provided. Whenever an employment relationship changes, because of an upgrade or switch from part time to full time, a new record is created. The third archive contains information on the firm, its industry code (3-digit), the municipality in which the firm is located and its post code. If a firm changes location or sector of activity a new record is created.

The INPS data also provide detailed information on country of birth (overall, 154 countries).[7]

The data exclude self-employed individuals or those employed in family businesses for which registration at INPS archive is not required. Both workers and firms in the data are individually identifiable and can be followed over time. Workers originally observed in Veneto who are subsequently employed anywhere else in Italy are also followed in the data. The absorbing state hence includes non-employment, death, movements to other countries (including the home country for non-natives), self-employment, public sector employment and informal employment. The original dataset includes information on around 3.6 million workers for a total number of approximately 12.5 million employment relationships in more than 1.1 million firms.

1.2.1 Immigrants in Veneto's Labour Market

While being one of the largest sources of immigration to the USA and the rest of America in the early twentieth century and a traditional source of internal migration up to the 1970, Veneto has witnessed a large influx of international migrants in the last thirty years, currently being one of the favoured destinations among international migrants to Italy. Between 1990 and 2001, the number of immigrants in the population increased almost three-fold, from around 50,000 to more than 140,000 out of a total population of 3.5 millions. In 2001 the share of migrant population in Veneto was about 4%, well above the national average of 2.3% (Anastasia, Gambuzza and Rasera 2001; Venturini and Villosio 2008).

Figure 1.2 plots the evolution of foreign workers presence in Veneto since 1975 based on INPS data: the share of migrants among formal non-agricultural private

[7] The data only refer to foreign born individuals, including legal immigrants with a work permit currently employed as formal employees. The data exclude all the undocumented migrants working in Italy, which are estimated to account for about 10% to 40% of the regular foreign workforce (Venturini and Villosio 2008). See Appendix B for a brief summary of immigration policies in Italy.

sector employees in Veneto started increasing rapidly after 1990, the highest increase being between 1995 and 2000, following two large regularizations of illegal immigrants. This pattern is in line with immigration trends in Italy: from 1970 to 2000 the number of foreign workers has increased from about 150.000 to 1.3 million.[8]

Figure 1.2 also shows that the origin of immigrants has varied significantly over the period considered: the share of immigrants from EU15 countries has decreased (from about 47% in 1975 to 16% in 2001), while the share of immigrants from the Balkans and North Africa has increased, the most numerous immigrants' groups in 2001 being Moroccans, citizens of former Yugoslavia and Albanians, respectively with shares equal to 12.7%, 9.4% and 7.3%.

Table 1.1 presents averages over the entire period of the main variables in the dataset by immigration status (migrants vs. natives). Immigrants represent about 7 percent of the individuals in the sample.[9] Since migration to Veneto is a recent phenomenon, most foreign workers appear in the last years of observation, partly explaining why the average length of employment spells is shorter among migrants than natives. About 58% of migrants who ever worked in Veneto are present in the last year of the dataset (the corresponding figure for natives is 45%), and the average length of the spell is half the one for natives. The shorter duration of job matches among migrants however is also indicative of migrants switching jobs more frequently. Indeed transition rates show that migrants have both higher exit and entry rates from and into employment than natives: the monthly exit rate for natives is 1.7% while for migrants this is 3.2%. Entry rates for natives and immigrants are respectively 1.68% and 3.14%, suggesting that immigrants are more mobile in the labour market and tend to end up in more precarious jobs than natives.

Table 1.1 also reports information on the gross weekly wage; values are expressed in real terms (Euros of 2003) and are comprehensive of all payments including

[8]For an extensive review of immigration trends to Italy see Ministero dell'Interno (2007).

[9]According to Venturini and Villosio (2008), in 2001 in Italy there were 1.4 million foreign workers, representing about 6% of the total workforce. This share in the northern regions was higher than the national average, being equal to 7.3%.

overtime and bonuses. Immigrants' weekly wages are lower than natives' by about 29 Euros, roughly 4%.

Migrants tend to be employed in low skilled occupations and in smaller firms, which pay lower wages and have fewer restrictions in firing decisions.[10] 72% of migrants are blue collars workers compared to 63% of natives; the average number of co-workers in the sample is 213 for migrants and 481 for natives. Immigrants are more likely to work for firms in which other migrants are also employed: the number of foreign co-workers is 26 for migrants and 13 for natives.

Table 1.2 explores key characteristics of firms and municipalities in the data. Veneto firms are in general very small: the average firm size is equal to about seven employees.[11]

The Table also reports values of two measures of segregation of migrants: the *dissimilarity* and the *isolation* indexes.[12] The dissimilarity index, also known as the Duncan index of segregation, tells us whether immigrants are evenly distributed over firms or municipalities. The index is defined as:

$$DI = \frac{1}{2}\sum_{i=1}^{N}|\frac{Migrants_i}{Migrants_{Total}} - \frac{Natives_i}{Natives_{Total}}|,$$

where i is the unit of analysis, i.e. the firm or the municipality of work, $Migrants_i$ is the number of all immigrants employed in unit i, $Migrants_{Total}$ is the number of all migrant workers in the population; $Natives_i$ is the number of Italian workers in unit i and $Natives_{Total}$ represents the total Italian workforce in the dataset. This index reports the share of migrant workers that would have to move to different firms (or cities) in order to produce a distribution that matches the one of natives. It ranges from zero, when all the units have the same relative number of migrants

[10] In Italy a law regulating employment relationships, the "Chart of Workers' Rights" (*Law No. 300: Statuto del Lavoratori*) of 1970, introduced norms that restrict firing decisions of firms with more than 15 employees. In case of unfair dismissals, firms are forced to take back the displaced employee and to pay him his full wage before the lay-off. Moreover firms are fined up to 200% of the displaced workers' original wage for the delayed payment of contributions.

[11] Italy is characterized by a multitude of small firms and few big companies; the Italian average firm size is equal to 10.5 employees (Bartelsman, Scarpetta and Schivardi 2003).

[12] Segregation is defined as the degree to which two or more groups live or work separately from one other (Massey and Denton 1988).

and natives, to one, i.e. complete segregation. Following Cutler, Glaeser and Vigdor (1999), values of this index higher than 0.6 imply high levels of segregation.

However, even if migrants evenly work in firms and cities relative to natives, it does not mean that they frequently interact with natives. For instance, immigrants can be evenly distributed among firms but have few contacts with natives if their share in the overall population is relatively large. The *isolation* index measures the exposure of migrants to natives, it indicates the amount of potential contacts and interactions between immigrants and natives within firms or cities. The index is defined as:

$$II = \frac{\sum_{i=1}^{N}(\frac{Migrants_i}{Migrants_{Total}} * \frac{Migrants_i}{Workforce_i}) - \frac{Migrants_{Total}}{Workforce}}{1 - \frac{Migrants_{Total}}{Workforce}},$$

where i is the unit of analysis and $Workforce_i$ is the number of all the workers in unit i irrespectively of the country of origin. The first term in the numerator, $E = \sum_{i=1}^{N}(\frac{Migrants_i}{Migrants_{Total}} * \frac{Migrants_i}{Workforce_i})$, is the typical *exposure* index (Massey and Denton 1988), which has been adjusted by subtracting the share of migrants in the total working population of Veneto, i.e. $\frac{Migrants_{Total}}{Workforce}$. Indeed, when immigrants in the population are few it would be impossible for them to be completely isolated, this adjustment then eliminates the effect arising from the overall size of the migrant population. The adjusted exposure index has eventually been rescaled by $1 - \frac{Migrants_{Total}}{Workforce}$ so that we get a measure of *isolation* ranging between zero and one. Typically, values of this index higher than 0.3 suggest that immigrants are highly isolated (Cutler, Glaeser and Vigdor 1999).

From Table 1.2 there is evidence of low segregation at municipality level, with a Duncan index equal to 0.25, meaning that about one fourth of the all migrants would have to move municipality in order to produce a distribution that matches that of the natives. The index substantially increases when the unit of analysis is the firm: more than half of migrant workers have to switch firm in order to have no segregation at the firm level. The same pattern applies to the isolation index, the level of exposure significantly increases when the unit of analysis is the firm, being the index equal to 0.27. In sum, despite the relatively low level of residential segregation, immigrants seem to be highly segregated at the firm level.

Figure 1.3 further explores segregation at the city level separately by country of birth; in this figure only the most numerous groups are included. Segregation increases when the Duncan index is separately computed by country of origin. The least segregated migrants come from France (25.2%) while the most segregated are from Ghana (45.3%). Dissimilarity between minority groups is also high: for example, workers from former Morocco are equally segregated from Italians (32.7%) as they are from Yugoslavians (31.9%).

1.2.2 Closing Firms and Displaced Workers

In the rest of this section I focus on displaced workers, i.e., those who lost their job because of a firm closure. Overall 16% of the firms do not survive to the last year of observation.[13] Closing firms are in general smaller than the rest as they employ on average 4.8 employees.

Of the 261,399 migrants ever observed in data in the period 1975-2001, 16,857 were laid off because of a firm closure. Some of them were displaced more than once, giving a total of 18,267 displacement episodes. Relative to the entire sample of workers, displaced workers are younger, more likely to be female, earn lower wages and more likely to be employed in unskilled occupations. Compared to natives, migrants have a higher propensity to be displaced: the share of workers displaced every month, i.e. the transition from employment to non employment due to firm closure, is 0.14% among migrants and 0.10% among natives. Not only is the monthly displacement rate higher for migrant workers but, conditional on displacement, re-employment probabilities are lower: among displaced workers

[13] A firm closure is recorded whenever a firm shuts down; in the dataset a specific variable indicates the (month) date at which a firm stops its business and thus disappears from the sample. This variable also distinguishes between real closure and other events affecting a firm's business other than closures, such as changes in the name and in the organisation, breaks up, mergers and acquisitions.

49% of the natives find a job in the first 3 months following a firm closure, while the same figure for migrants is 46%.

Figure 1.4 explores the effect of displacement episodes on subsequent employment probabilities of migrant displaced workers. It plots the coefficients of a regression in which the employment probability is a function of individual characteristics, such as age and gender, as well as time exposure dummies for each of the 36 months before and after the closure.[14] While there is no clear pattern before the displacement episode, Figure 1.4 shows a strong persistence of displacement, on subsequent employment outcomes; even after 36 months, the probability of finding a job is negatively affected by the firm closure. Regressions are run separately for immigrants and natives: the persistence of the displacement effect does not vary by immigration status, however it seems that natives recover slightly faster than migrants after job loss. For both immigrant and native workers the consequences of displacements on successive labour market performances are long lasting.

1.3 Empirical Strategy

This section presents a linear-in-means model in which the re-employment probabilities of unemployed workers depend on the both employment rate and the observed characteristics of network's members:

$$y_{it} = \beta_0 + \beta_1 \overline{y}_{-it} + \overline{\mathbf{x}}'_{-it}\beta_2 + \mathbf{x}'_{it}\beta_3 + u_{it} \tag{1.1}$$

where y_{it} is a dummy variable equal to one if worker i is in employment at time t; $\overline{y}_{-it}$ denotes the network's employment rate at time t and $\mathbf{x}$ is a vector of individual characteristics. For each individual i, a *network* is defined as the group

[14]The estimated equation is $y_{its} = \alpha + \sum_{k=-36}^{+36} \delta_k D_{ik} + \lambda_i + u_{its}$. D_k are dummies for a worker's time exposure for each month t before and after displacement, i.e. $D_k = I[t - s > k]$, where s is the displacement date. All regressions include individual fixed effects, standard errors are robust.

of past co-workers from the same country of origin in the five years preceding the displacement.

The coefficient β_1 captures the endogenous social interaction effect. Least squares estimates of this coefficient can be biased because of correlated effects i.e. the presence of institutional environments or common unobserved individual characteristics that lead to spurious correlations among group members' behaviours. This is for example the case of aggregate supply and demand shocks that equally affect workers from the same country of origin or those in a specific local labour market.

In an attempt to control for such correlation, regressions include a set of controls for observed workers' and environment characteristics, such as nationality, time and municipality of first work in Veneto. As long as the network measure is worker-specific, it is possible to compare re-employment probabilities of individuals with different network employment rates who are otherwise identical because of their country of origin and the initial location of work.

Another source of potential endogeneity arises from non-random sorting: agents might self-select into reference groups according to unobservable characteristics that simultaneously influence group membership and individual behaviour.

Finally, reflection might lead to biased OLS estimates. In a network composed of two workers, i and j, i's behaviour will influence j's behavior and vice versa, implying that OLS estimates of equation (1.1) will pick up more than the causal effect of j's on i's behaviour (Manski 1993).

The identification of the endogenous effect is still possible by means of instrumental variables, where the instrument is an exogenous variable affecting j's outcome variable directly and i's outcome only through the endogenous social interaction. Following a well-established literature that shows long-term effects of displacement (von Wachter and Bender 2007), in the rest I use past co-workers' displacement episodes as an instrument for their current employment status. In particular, I instrument a network member's employment status by his own displacement episode between the time the connection with pivotal worker i was established and the month before the pivotal individual's displacement episode.

In practice I augment equation (1.1) with a dummy variable z_{it} equal to one if an individual was ever displaced up to period t. Clearly, because I restrict the sample to pivotal individuals i who have been displaced, the variable z_{it} is equal to one in the main equation. The first stage equation then takes the following expression:

$$\overline{y}_{-it} = \gamma_0 + \gamma_1 \overline{z}_{-it} + \mathbf{x'}_{it}\gamma_2 + \overline{\mathbf{x}}'_{-it}\gamma_3 + e_{it} \tag{1.2}$$

where $\bar{y}_{-it}$, the network employment rate, is regressed on the fraction of network members who were ever displaced between the time they first worked with individual i and time t.

This instrumental variable estimate of the social interaction effect will be consistent if, as it seems plausible, firm closures are uncorrelated with a worker's characteristics that simultaneously affect both his and his network members' latent employment outcomes. Under this assumption, the instrumental variable approach will eliminate any residual endogeneity arising from unobserved network's characteristics.

1.4 The Effects of Networks: Empirical Results

In the rest of the analysis, I focus on networks that are created at most five years before the displacement. Because of this, I drop the first five years of observation in the dataset (1975 to 1979) hence focusing on job loss episodes that occur not earlier than January 1980. Displacements occurring in the last three years (1999 to 2001) are also excluded so that workers can be followed for up to 36 months after job loss.[15]

[15] If a worker experienced more than one closure, I only consider the first episode, as the subsequent episodes are likely to be correlated with the first one.

In order to solve for the reflection problem, I define the dependent variable y_{it} in equation (1.1) as a dummy variable equal to one for non-employment spells starting at t, which are concluded within a given time span (e.g. 36 months); while $\bar{y}_{-it}$, the network employment rate, is the share of network members employed at the time of i's displacement episode. The instrumental variable, $\bar{z}_{-it}$, is thus the share of network members that have experienced a firm closure between the time they first met worker i, up to the month before worker i's displacement episode. This instrument is thus worker specific and it solves any potential reverse causality issue: since contemporaneous firm closures may be correlated, the instrument only considers job loss experienced by group members before individual i's displacement episode.

Eventually, the sample analysed is composed of 10,738 workers who experienced a firm closure between January 1980 and December 1998. Excluding closures occurring in 1999, 2000, and 2001 decreases the sample size to 14,317. Moreover, by dropping closures happening in the first 5 years of the dataset, the number of displaced immigrants becomes equal to 13,194. Finally, workers who experienced a closure while they were employed at the same time in another firm are excluded from the sample of displaced workers.

1.4.1 Baseline Specification

Table 1.3 reports estimation results of model (1.1) and (1.2); controls include age, country of origin, and gender dummies for worker i plus the averages of the same variables for network's members and a set of dummies for the size of the network. In addition, dummies for the month of displacement are added to the regressions. Standard errors are clustered by country of origin.[16]

[16] This is the most restrictive specification: clustering at country level increases standard errors and it thus affects the significance of the coefficients. A less restrictive specification by country of origin interacted with the month of displacement has been tested in the regressions: the magnitude of standard errors decreases. The tables only report standard errors clustered by country of origin.

Column (1) of Table 1.3 reports baseline IV estimates: the endogenous interaction coefficient, β_1, is positive and statistically significant at 1% level; this result suggests that past co-workers' employment status has thus a positive effect on the displaced workers' probability of finding a job in the 36 months after firm closure. This first specification includes month of displacement dummies; column (2) of the same Table additionally controls for the interaction between country of birth and the month of displacement, accounting for unobservable shocks that equally affect migrants from the same country that have been laid off at the same time. As country specific shocks are absorbed, the coefficient of interest falls in magnitude and significance but it remains positive and statistically significant.

Consistent with Figure 1.4, first stage regression estimates confirm the strong predictive power of the instrument; the bottom rows of Table 1.3 show that these estimates are very precise, being the value of the F-test (40.74) reasonably high.[17]

To further account for endogenous location choices, column (3) includes the interaction between nationality, date of displacement and the first municipality of work in Veneto; in practice I am comparing two individuals from the same country of origin, who started working in the same municipality and who have experienced a firm closure at the same time. Within-country and within-municipality comparisons control for any spurious correlation due to unobservables that affect all individuals from the same country that started working in the same local labour market.[18]

The empirical evidence shows that social spillovers still persist: as more restrictive controls are added both the significance and the magnitude of the endogenous effect increase. The more members employed in the network at the time of displacement, the higher the re-employment probability of displaced co-workers within 36 months following firm closure. The coefficient of the social effects tells that a 10 percentage point increase in the network employment rate raises the probability of finding employment within 36 months after job-loss by 5.7 percentage points. In other

[17]Coefficients of the first stage regressions exhibit a positive sign because of the way the regression's sample is constructed.

[18]Because of non-random sorting, controls for the first city of work should be less endogenous with respect to subsequent cities, including the one of displacement.

words, a one standard deviation rise, i.e. about 28 percentage points, in the network employment rate leads to a 34 percentage point increase in the 36 months re-employment probability.[19]

Social networks have thus a beneficial effect on re-employment probabilities of their displaced group members. Moreover, estimates of the endogenous effect are significant and positive in every specification adopted.

OLS regressions are presented in Appendix A: coefficients are always smaller than the ones reported in Table 1.3, suggesting that OLS estimates are downward biased. One possible explanation for this bias could be negative sorting into groups: high ability immigrants prefer not to rely on their co-national past coworkers. The next subsection aims at exploring the heterogeneity of the network effect by running separate regressions according to displaced workers' characteristics.

1.4.2 Heterogeneity of the Network Effect

Results in the first three columns of Table 1.3 impose that the social effect is constant across different types of migrant workers; however, it is reasonable to think that this network effect differs according to workers' characteristics, such as experience and tenure in the labour market.

As highlighted by several studies Edin, Fredriksson and Aslund (2003), less experienced immigrants are more prone to rely on their acquaintances, being thus the ones who benefit the most from the help of their co-workers. In order to test this hypothesis, I run separate regressions in which the sample of displaced workers is split according to their occupation and tenure at the time of displacement.

In columns (4) and (5) the sample is divided on the basis of the occupation of the pivotal individuals at the time of firm closure. Blue collar workers are analyzed in column (4), they represent about 70% of the whole sample; while in column (5)

[19]In other terms, one more additional worker employed in a displaced worker's group at the time of displacement increases his chances of finding a job in the next 36 months following a firm closure by 12 percentage points.

I retain occupations other than blue collars, such as white collars and managers, accounting for the remaining 30% of displaced migrants.

Estimates indicate that immigrants employed in *unskilled* occupations are the only ones for which the endogenous social interactions are positive and significant: the coefficient of the network employment rate is equal to 0.54 and statistically significant at 5% level. There is no significant effect for other categories of workers, as shown by results in column (5).

To further explore the heterogeneity of the network effect, I focus on migrants' tenure in the Italian labour market. I define *low-tenured* immigrants those who have been employed less than 20 months prior the job loss, i.e. the median of the distribution of months in employment. The coefficient in column (6) is still positive and it increases in both significance and magnitude: a 10 percentage point raise in the network employment rate increases the 36 month re-employment probability of low tenured immigrants by about 9 percentage points. There is no significant effect for more experienced workers, as shown in column (7).

Immigrants' use of their acquaintances may also vary depending on their country of origin. Whenever labour markets function imperfectly, non-market institutions, such as social networks, may emerge in order to contrast market failures. Personal contacts then represent the major source of job information and support for immigrants coming from less developed countries. Workers from those countries may systematically rely on their social networks also in the host country. I therefore split the sample in two subgroups depending on whether a worker's country of origin is an OECD member state. The coefficient of the network employment rate is positive and significant only when regressions are run for non-OECD countries; this result suggests that workers from least developed countries make a wide use of their personal contacts even after they moved to Italy.

Regressions in Table 1.3 only analyse the network effect on re-employment probabilities within 36 months following a lay-off. However this effect may vary according to the time window considered. Figure 1.5 plots re-employment probabilities of displaced individuals in each of the 36 months following the displacement; because of censoring, the graph does not include displaced workers who have not found a job within 36 months, i.e. about 27% of the sample. Almost 30% of displaced migrants found a job within the very first month of unemployment, while only a small portion of workers are still non-employed after the first year following the lay off.

Figure 1.6 reports coefficients of the network employment rate from 36 regressions in which the dependent variable is, in turn, the cumulative re-employment probability from one to 36 months after job loss. As in column (3) of Table 1.3, I control for the interaction between the country of origin, the time of displacement and the first city of work; standard errors are clustered by country. The vertical lines in the graph depict the 90% confidence intervals.

The estimated coefficients actually change according to the different time intervals considered: they are always positive but they become statistically significant only after the 20th month since job loss. The effect appears particularly high within the first months following the displacement even though it is not statistically significant. After the 20th month, the social effect stays positive and significant up to the 36th month.

One possible interpretation of these results is that immigrants use their personal contacts as a *last resort* when they are not able to find a job through the formal channel. However a delayed effect of networks can be explained by the fact that networks are particularly helpful for immigrants with limited access to employment opportunities, as shown in Table 1.3. Low-skilled and unexperienced workers are the ones who struggle the most after firm closure, hence they may take long time to find a job.[20]

In order to further investigate this issue I look at the timing of the social effect separately for workers with low and high tenure in the labour market. If networks represent a last resort in job search, I should not observe any differences in the timing of the network employment rate effect between the two groups. Figure

[20]Figure A1 in the Appendix plots re-employment probabilities of displaced individuals in each of the 36 months following the displacement by tenure in the labour market. Among low tenured displaced workers 41% do not find a job, while the same figure for high tenured is about 25%.

1.7, plots the coefficients of the network employment rate on the cumulative re-employment probabilities from one to 36 months after job loss for low and high tenured workers respectively. Regression coefficients exhibit different values and patterns for the two groups. Low tenured workers have a beneficial and positive effect from the employment status of their past co-workers after the first 10 months since job loss; conversely, high tenured benefit from the employment status of their co-workers only in the very first months after firm closure, the effect then becomes insignificant. The interpretation of networks as a last resort seems to be not supported by these results; on the contrary, the delayed effect of networks can be interpreted as a composition effect: low skilled displaced workers are the ones who need more time to find a job and, at the same time, they are also the ones who rely more on their personal contacts while looking for a job.

1.4.3 Effects of Other Groups

So far *networks* have been defined as groups of co-national past co-workers, relying on the assumption that immigrants tend to interact mainly with workers from the same country of origin. This section investigates whether co-workers from different nationalities provide the same valuable information in job search; in particular, I test if the employment status of past co-workers other than co-nationals affects the 36-month re-employment probability of displaced migrants.

Table 1.4 presents estimates from regressions in which the re-employment probability of a displaced worker depends on the employment rate of past co-workers from other countries of origin. The first two columns of the Table focus on groups composed of immigrants from other foreign countries (i.e. *non-nationals*), while in the last two columns networks only include native past co-workers (i.e. *Italians*).[21] IV regressions include average characteristics of past co-workers, as well as dummies for the size of the network. Controls are the interaction between the country of origin, the month of displacement and the first city of work.

[21] From now onwards I will refer to co-workers from the same country of origin of the pivotal displaced worker as *co-nationals*, the ones from different countries of origin (excluding Italians) as *non-nationals* and *Italians* for the natives past co-workers.

The estimate of the effect of non-national co-workers' employment status on the individuals' re-employment probability is positive but not significant in column (1), where I only include the interaction between the country of origin and the month of displacement. When I additionally control for endogenous location choices, i.e. column (2), the sign of the coefficient turns negative but it is still not significant. It is also interesting to notice that the coefficients of the employment rate of the non-nationals are always smaller in magnitude than the ones of the co-nationals found in Table 1.3. These results indicate that there is no evidence of significant social interactions among co-workers of different nationalities; further, the negative sign for the coefficient in column (2) suggests that immigrants, who used to be co-workers but from different nationalities, rather compete for the same job vacancies.

Columns (3) and (4) of Table 1.4 analyse networks composed of Italian past co-workers. Regressions still compare two individuals from the same country of origin, who have experienced a firm closure at the same time, however the network does not include any migrant past co-workers. Results are similar to the ones found when non-nationals are taken as reference group: the coefficient of the social effect is positive but not significant. As more restrictive controls, i.e. the first city of work, are added, the sign of the coefficient turns negative but it is still not significant. Interestingly, the estimated coefficients when the reference group is only composed of Italians are always smaller than the ones found when immigrants are included in the reference group. This difference in magnitude may indicate that interactions between natives and immigrants are occasional, either because of preferences (or tastes) or because they end up working in different occupations or firms. First stage regressions again confirm that the instrument has a strong predictive power, which is particularly performing when natives are considered as a reference group.[22]

[22]Table A3 in Appendix A provides supplementary robustness checks. I first run regressions in which I include a control for the industry of displacement: estimates of the social effect stay significant and positive when network members are co-workers from the same country of origin; not significant effects are found for other network members, both foreigners and natives. Moreover I run regressions in which I simultaneously include the network employment rate of co-national, non-national and native past co-workers: only the coefficient of network members from the same country of origin is positive and statistically significant.

Results in Table 1.4 might also be considered as a test validating the identification assumptions developed in Section 1.3. Indeed, estimates in Table 1.3 may still be driven by omitted characteristics that simultaneously affect individual i's probability of finding employment and his co-workers' probability of displacement rather than a genuine social effect; for instance, if low ability individuals self select into firms with a high probability of closure, the identification assumption would be invalid, as firm closures affecting group members could be correlated with unobserved characteristics of the pivotal displaced individual.

Past co-workers from other countries are likely to share the same unobserved characteristics as co-national co-workers but they are unlikely to provide valuable information in job search; finding a significant positive effect also for co-workers from different nationalities would suggest sorting along unobservables, possibly driving the estimates of social effects among co-nationals in Table 1.3.

Regressions in Table 1.4 provide statistically not significant coefficients in any specifications adopted: the positive social effect found for co-national networks is not biased by omitted variables affecting workers that have worked together in the same firm. If there were sorting, generating spurious correlation leading to a significant network effect as in Table 1.3, then the effect of non-national past co-workers would have been significant. These results then confirm the validity of the instrument used, which manages to solve potential biases coming from the endogenous group formation.

1.4.4 Social Effects among Natives

In previous Sections I only focus on interactions among immigrants, however Figure 1.1 shows that in every European labour market native workers also rely on their personal contacts while looking for a job. Moreover, previous studies report that a positive network effect exists among natives too; Cingano and Rosolia (2012), using a reduced version of these data, provide evidence of significant and robust network effects on unemployment duration of native workers. Similarly, Glitz (2013) using data on employees in Germany, finds a strong positive effect of a higher employment rate in a worker's network on his re-employment probability after displacement.

This section explores whether endogenous interactions take place among natives and how this social effect compares to the one found for immigrants. Columns (5) and (6) of Table 1.4 provides IV estimates of the effect of the employment rate of network members on the 36 month re-employment probability of a sample of native displaced workers. Controls include age and gender dummies for worker i plus the averages of the same variables for network's members and a set of dummies for the size of the reference group.[23] In column (5), only dummies for the month of displacement are added to the regressions.

The effect is positive and significant: a 10 percentage point increase in the employment rate of past co-workers increases re-employment probability of displaced native workers by about one percentage point. A higher employment rate of past co-workers is beneficial also for displaced native workers. When more restrictive controls are added to the regressions (column 6), i.e. dummies for the first city of work, the effect does not change in significance and it slightly increases in magnitude, being now the coefficient equal to 0.109.

The existence of a positive social effect for natives is in line with findings in Cingano and Rosolia (2012): they found that a one standard deviation increase in the network employment rate reduces unemployment duration by almost 8%.

From these results, we can draw two conclusions that are consistent with the empirical evidence of Figure 1.1. First, social interactions take place among Italian employees, suggesting that also native co-workers interact and help each others in job search. Second, immigrants rely more on the help of their acquaintances than natives: the size of the network employment rate coefficient for immigrants is higher the size of the one for natives, i.e. 0.57 versus 0.11.

[23]Country of origin dummies are included but automatically dropped in the regressions as all the displaced individuals are Italian workers and thus share the same nationality. Standard errors are thus clustered by the date of displacement.

1.5 Network Mechanisms and Segregation: Empirical Analysis of Post-Displacement Outcomes

1.5.1 Possible Mechanisms behind the Social Effect

This last section attempts to shed light on the possible mechanisms behind the estimates of the social effect previously found. Among several possible explanations, a positive network effect can arise from two different channels: *information* and *norms* (Bertrand, Luttmer and Mullainathan 2000).

According to the information story (Calvo-Armengól and Jackson 2004), the more people employed in the network, the higher the probability of finding a job as the arrival rate of job offers increases. If employed, network members are better informed about job vacancies in firms or municipalities in which they work; moreover, employed members are also more likely to share their sources of job information, such as previous or current employers, with unemployed members. Therefore the higher the employment rate of the network, the lower the competition within the network for job openings and thus the higher the arrival rate of offers for displaced migrants.

Similarly, social norms can lead to a positive network effect on re-employment probabilities: as more members of the network are employed, unemployment may turn into a social stigma hence pushing displaced workers to rapidly exit from unemployment. A high network employment rate then may act as a sort of peer pressure on displaced migrants.

Table 1.5 provides estimates of the of the network employment rate on different outcome variables such as the firm and the municipality in which displaced immigrants find job after firm closure. Investigating where displaced immigrants end up after the displacement episode helps us understanding the mechanism behind the social effect.

The first outcome variable looks at firms in which the pivotal worker is re-employed after his own displacement. Firms are divided into two groups: firms in which at least one member of the network, i.e. a co-national past co-worker, has ever worked before individual i's displacement episode, i.e. *connected firms*; and firms in which no past co-worker has ever been employed, i.e. *non-connected firms*.[24]

In column (1) of Table 1.5, the dependent variable is the probability of working in a connected firm; the coefficient is positive and significant at 10% level implying that a 10 percentage point increase in the network employment rate increases the chances of displaced workers of finding a job in connected firms by 5.4 percentage points. In column (2) the outcome variable is the probability of finding a job in non connected firms: the coefficient is still positive but not significant and it is also smaller in magnitude than the one found in column (1). Note that these coefficients sum up to the net total effect found in column (3) of Table 1.3, i.e. 0.574.[25]

Past co-workers may also hear about job openings in municipalities in which they currently work or in which they have worked in the past; thus, they may help their unemployed network members by placing them in municipalities in which they have a connection. Regressions reported in Columns (3) and (4) of Table 1.5 look at the effect of the network employment rate on the municipality in which the displaced migrant is employed after job loss; in column (3) the dependent variable is the probability of working in a *connected* municipality, where at least one past co-worker has ever been employed. Results are strongly positive and significant at 1% level, the coefficient of the social spillovers predicts that a 10 percentage point increase in the network employment rate increases the probability of working in connected cities by 7.9 percentage points. Conversely, the estimate of

[24] The econometric specification controls for the interaction of the country of origin, month of displacement and the first city of work. As in the previous section, the employment rate of network members is instrumented with displacement episodes experienced by group members before worker i's job loss. With respect to regressions in Table 1.3, only the dependent variable has changed therefore first stage regressions are the same as the ones reported in column (3) of Table 1.3.

[25] If a worker does not find a job within 36 months since job loss, both outcome variables, the probability of finding a job in connected and in non-connected firms, take a value equal to zero.

the network employment rate on the probability of finding a job in a non-connected municipality is negative but not significant.

The last columns of Table 1.5 investigate the effect of past co-workers' employment status on the probability of working in industries in which displaced immigrants have a connection, i.e. in which at least one network member has worked in the past. Again, the effect is positive and significant: stronger networks will help unemployed immigrants to get a job in *connected* industries.

As the employment rate of the network raises, displaced migrants are more likely to work after job loss in firms, municipalities and industries in which past co-workers have a connection. These results are consistent with the information transmission story. Each network has a pool of job information's sources, represented by connected workplaces; as more people in the network are employed, the higher the probability of hearing about job vacancies and the higher the probability that employed members will pass this information to the unemployed.

Interpreting these results through the lenses of the social norm channel is more difficult; this story predicts that as the employment rate of network increases, immigrants will exit the unemployment faster. There is no implication about the place of work in which displaced migrants will find a job. In addition, the social norm theory is not consistent with the delayed effect of the social effect found in Figure 1.6.

1.5.2 Networks and Segregation

The last part of this work analyses whether networks push immigrants to cluster together in the same local labour markets. Previous results show that immigrants pass information to their unemployed network members about job vacancies in *connected* workplaces. This result may also suggest that as the network employment rate rises, so do the probability of being employed in firms in which other immigrants from the same country of origin are employed, eventually increasing the level of segregation.

To further explore this issue, Table 1.6 reports a set of regressions in which the dependent variable is the probability of finding a job in firms in which at least one migrant worker is employed. I then distinguish between workers from the same country of origin and workers of different foreign nationalities.

The first column reports results from a regression in which the dependent variable is the probability that a displaced migrant ends up working with at least one co-worker (new or past) from the same country of origin in the 36 months after his own displacement episode. The coefficient is positive and significant: as the network employment rate increases by 10 percentage points, the probability of ending up working with at least one co-national coworker increases by 7.7 percentage points. In column (2) I explore whether the network employment rate has any effects on the probability of finding a job in firms in which no co-national worker is employed, the effect is negative but not statistically significant.[26]

This positive effect may be due to the fact that immigrants are employed in firms that systematically hire foreign workers; column (3) then looks at the probability of finding a job in firms in which at least one immigrant, who is either a new or a past co-worker of a different nationality, is employed; the effect of the network employment rate is positive but not significant; it is also smaller in magnitude than the coefficient in column (1). Again, the effect on the probability of finding a job in firms in which no immigrant from other nationalities is employed is not significant and very small in magnitude.

A higher network employment rate then increases the probability that displaced immigrants will be employed by firms in which other immigrants from the same country of origin work, potentially increasing the level of segregation at the workplace.

I thus explore whether the use of networks by immigrants can explain immigrant segregation and clustering in the workplace. First, I compute for each nationality

[26]Note that the two coefficients sum up to the network effect found in column (3) of Table 3.

the dissimilarity index at firm level over the period 1980-2001, which is defined as:

$$DI_g = \frac{1}{2}\sum_{i=1}^{N}|\frac{Migrants_{g,i}}{Migrants_{g,Total}} - \frac{Workforce_{-g,i}}{Workforce_{-g,Total}}|,$$

where i is the firm and g is the country of origin. $Migrants_{-g,i}$ is the number of immigrants from country g employed in firm i; $Workforce_{-g,i}$ is the number of workers, natives and immigrants other than the ones belonging to group g (i.e. $-g$), in unit i. $Workforce_{-g,Total}$ represents the total workforce in the dataset but immigrants from group g. The dissimilarity index gives me a measure of firm segregation for every immigrants' sending country. I then plot these dissimilarity values with the estimated coefficients of the network employment rate from regressions of model (1) and (2) separately run for each country of origin. Figure 1.7 shows a positive relationship between the social effect and the degree of dissimilarity by nationality: immigrants that are positively affected by the employment status of their co-national co-workers are also the ones who are highly segregated.

Similarly, Figure 1.8 explores the relationship between the network effect and another measure of segregation: the isolation index; this is again computed for every single sending country and it is defined as:

$$II_g = \frac{\sum_{i=1}^{N}(\frac{Migrants_{g,i}}{Migrants_{g,Total}} * \frac{Migrants_{g,i}}{Workforce_i}) - \frac{Migrants_{g,Total}}{Workforce}}{1 - \frac{Migrants_{g,Total}}{Workforce}},$$

where i again is the unit of analysis and g is the country of origin. $Workforce_i$ is the number of all the workers in unit i irrespectively of the country of origin.

The Figure suggests that whenever immigrants are largely exposed to other workers from the same country of origin, the magnitude of the network effect increases. Clearly this analysis does not have any causal implications; at this stage it is hard to tell whether the effect of network increases because of segregation. For instance the social effect may increase because social ties are tighter in segregated migrant communities; on the other hand, immigrants, who largely rely on networks, end up working in segregated firms. These findings however provide evidence of the positive correlation between the use of networks and segregation: the network effect increases for immigrants belonging to migrant groups that are relatively segregated in the Veneto labour market.

1.6 Concluding Remarks

The aim of this work has been to provide consistent estimates of the causal effect of past co-workers employment status on displaced immigrants' job search outcomes. For this exercise I use matched employer-employee micro data from the administrative records of the Italian Social Security Administration (INPS) for the Italian region of Veneto; the dataset covers the universe of private non-agricultural dependent employment relationships between January 1975 and December 2001.

To deal with several identification issues, such as reflection and endogenous group formation, I use displacement episodes of past co-workers as an instrument for their current employment status. As long as firm closures are uncorrelated with a worker's characteristics that affect both his and his network's labour market outcomes, this instrumental variable approach will lead to consistent estimates of the effect of interest. To further account for correlated effects, such as labour demand and supply shocks, controls for the time of displacement, the country of origin and the first municipality of work are included in the regressions.

The empirical analysis suggests three main conclusions. First, the net effect of migrant networks on re-employment probabilities is positive: a 10 percentage point increase in the network employment rate raises the probability of finding employment within 36 months after job loss by 5.7 percentage points. The effect of past co-workers from the same country of origin is positive and significant in any specifications adopted. The social effect becomes negative and not significant when I consider as a reference group past co-workers from different countries; I take this last finding as a validation of the empirical strategy.

Second, the network effect is particularly relevant for immigrants with limited job offers in the labour market, such as low skilled and low tenured workers. Moreover, estimates show that the magnitude of the social effect increases after the 20th month of job search: immigrants at the bottom of the skills distribution are the ones who rely more on the help of their past co-workers.

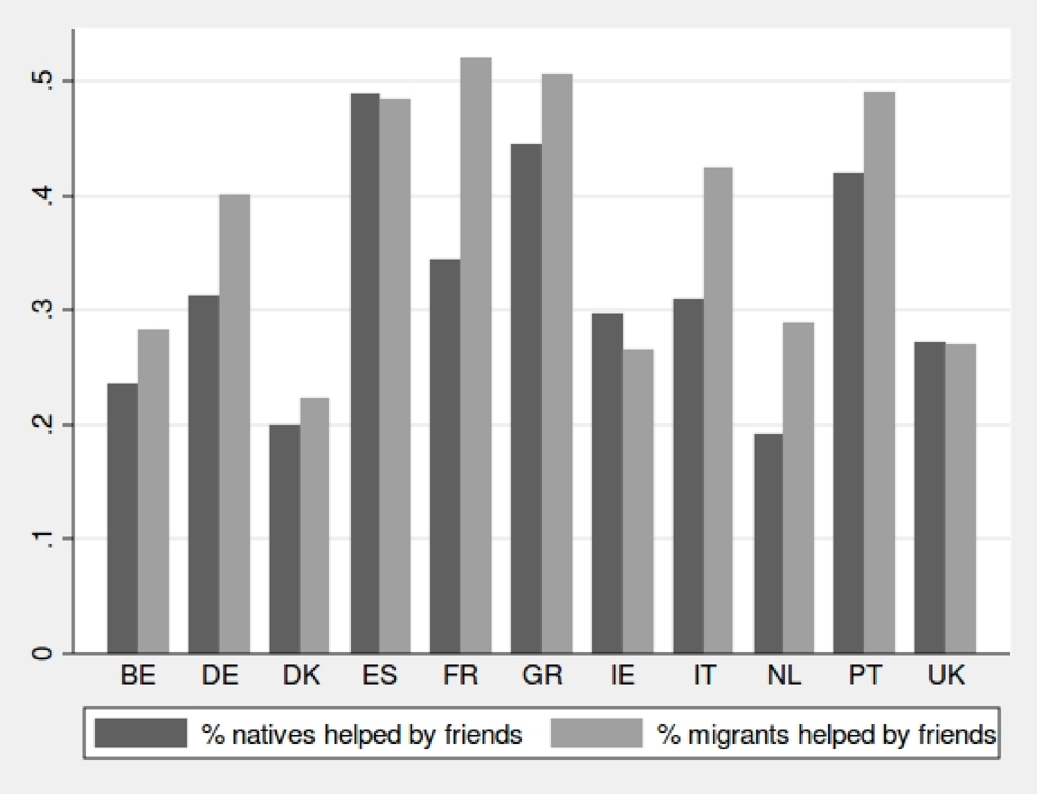
Third, the analysis of post-displacement outcomes shows that employed network members provide displaced co-workers with information about job vacancies in cities and firms in which they have worked, i.e. *connected* workplaces. The information transmission mechanism described by Calvo-Armengól and Jackson (2004) seems to be the prevailing one: the higher the employment rate of the network, the lower the competition within the network for the same sources of job information.

This work also presents empirical evidence of the positive correlation between the magnitude of the network effect and the level of immigrant workplace segregation. As the network employment rate increases, displaced migrant workers are more likely to find a job in firms in which at least one immigrant of the same nationality is employed, potentially increasing the level of exposure to co-workers from the same country of origin.

The evidence of a positive social effect suggests that interactions between employees coming from the same country of origin are an important channel through which migrants find a job. However networks can eventually push immigrants to cluster into the same workplaces.

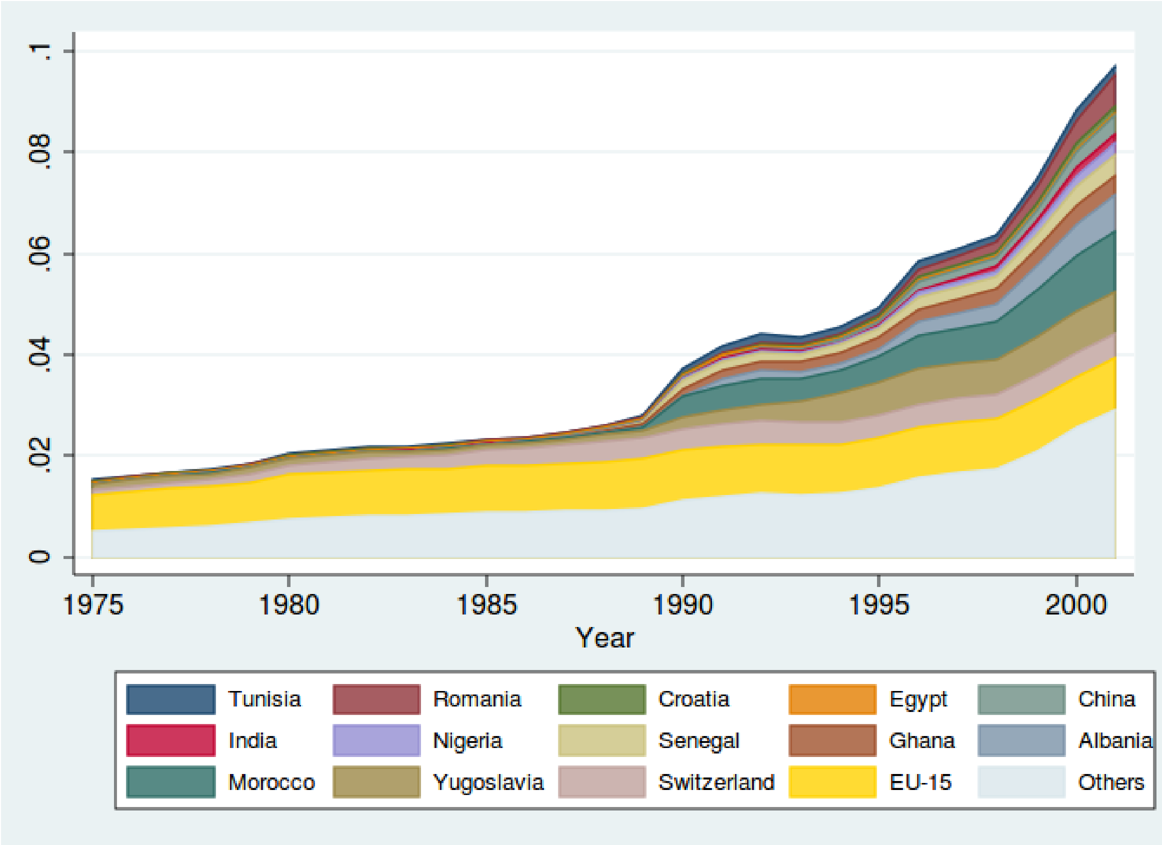
Tables and Figures

FIGURE 1.1: Share of employees who found their current job through personal contacts



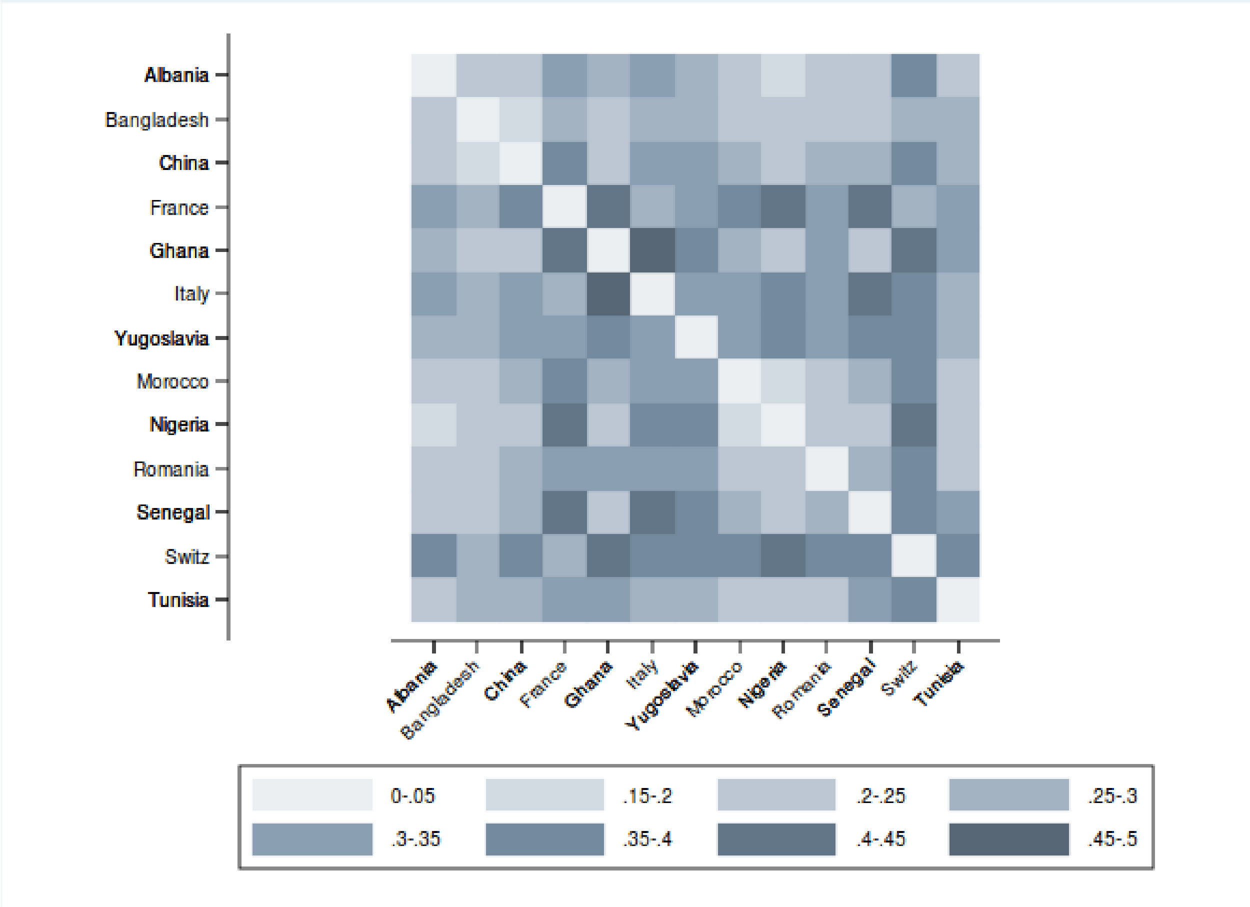
Notes: author's calculations on ECHP data for the period 1994-2001. The sample includes private sector dependent employees aged 16-64; Luxembourg, Sweden, Finland, Austria and Denmark are excluded from the analysis as they are not covered in all the waves. The precise question asked in this survey is: *"by what means were you first informed about your current job?"*. Respondents then have six different alternatives, which include *"friends, family or personal contacts"*.

FIGURE 1.2: Share of migrant workers in Veneto, 1975 - 2001



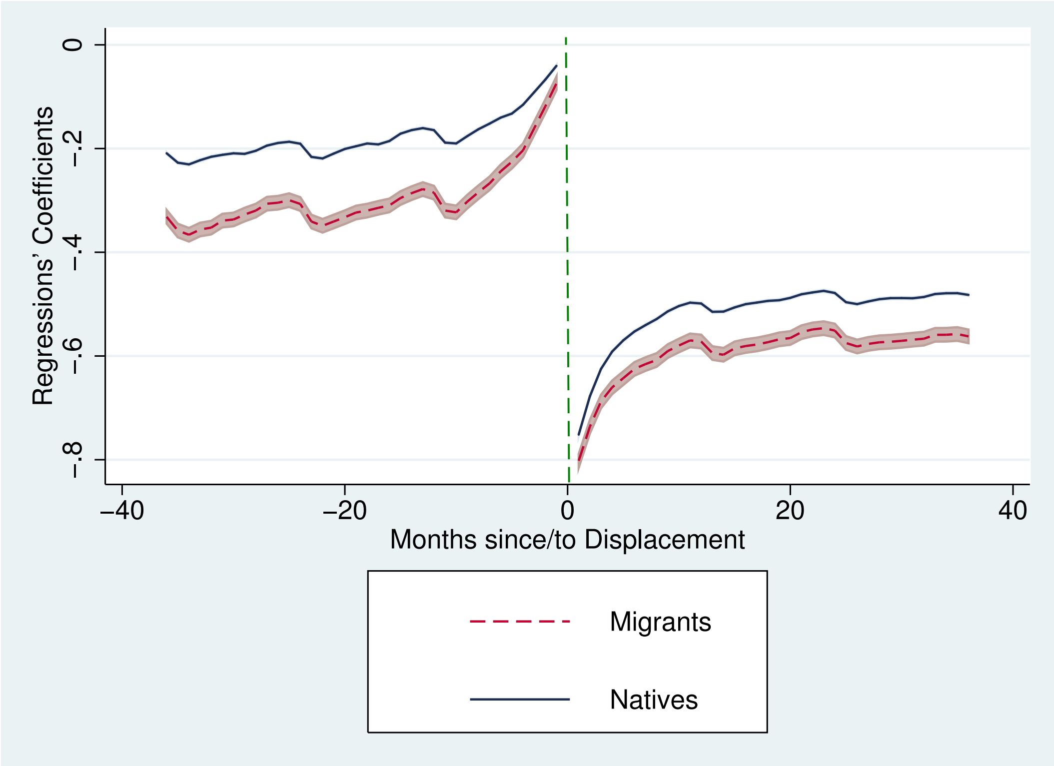
Notes: author's calculations on INPS data for the period 1975 - 2001. Each shaded area represents the share of immigrants from the corresponding country of origin on the overall population.

FIGURE 1.3: Duncan index of segregation at municipality of work level



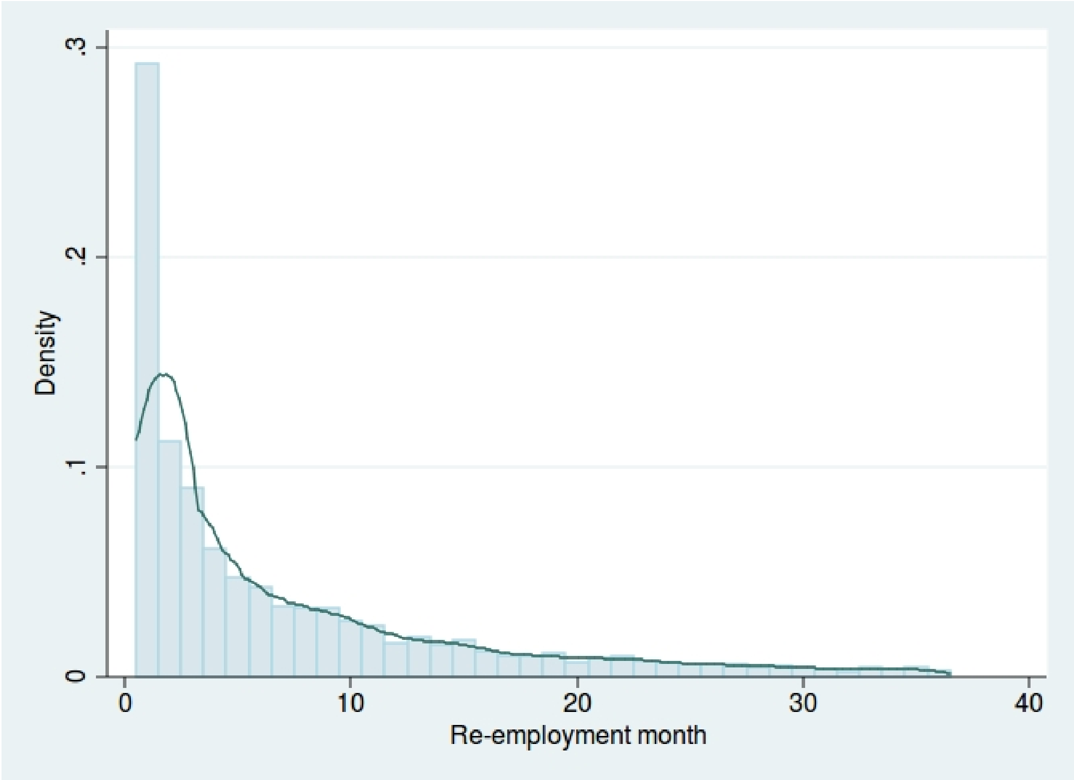
Notes: this Figure is based on INPS data for the period 1975-2001. Each square in the *heat map* represents the value of the dissimilarity index of each country of origin from anyone other.

Figure 1.4: The effect of displacements episodes on employment probabilities



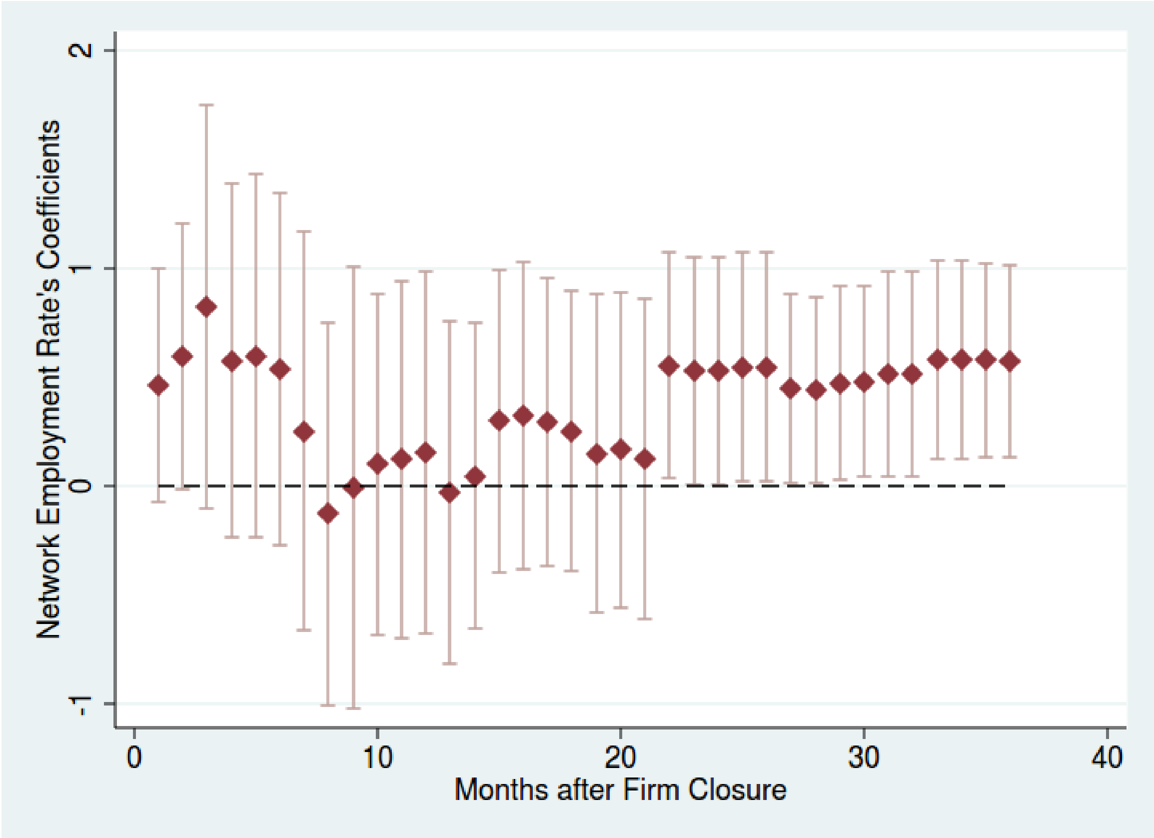
Notes: the sample includes displaced workers only. The estimated equation is $y_{its} = \alpha + \sum_{k=-36}^{+36} \delta_k D_{ik} + \lambda_i + u_{its}$. D_k are time exposure dummies for each of the 36 months before and after the closure. i.e. $D_k = I[t - s > k]$, where s is the displacement date; two separate sets of regressions have been run for migrants and natives. Standard errors are robust. The shaded areas in the figure represent the 95% level confidence intervals.

Figure 1.5: Re-employment probabilities by month (up to 36 months)



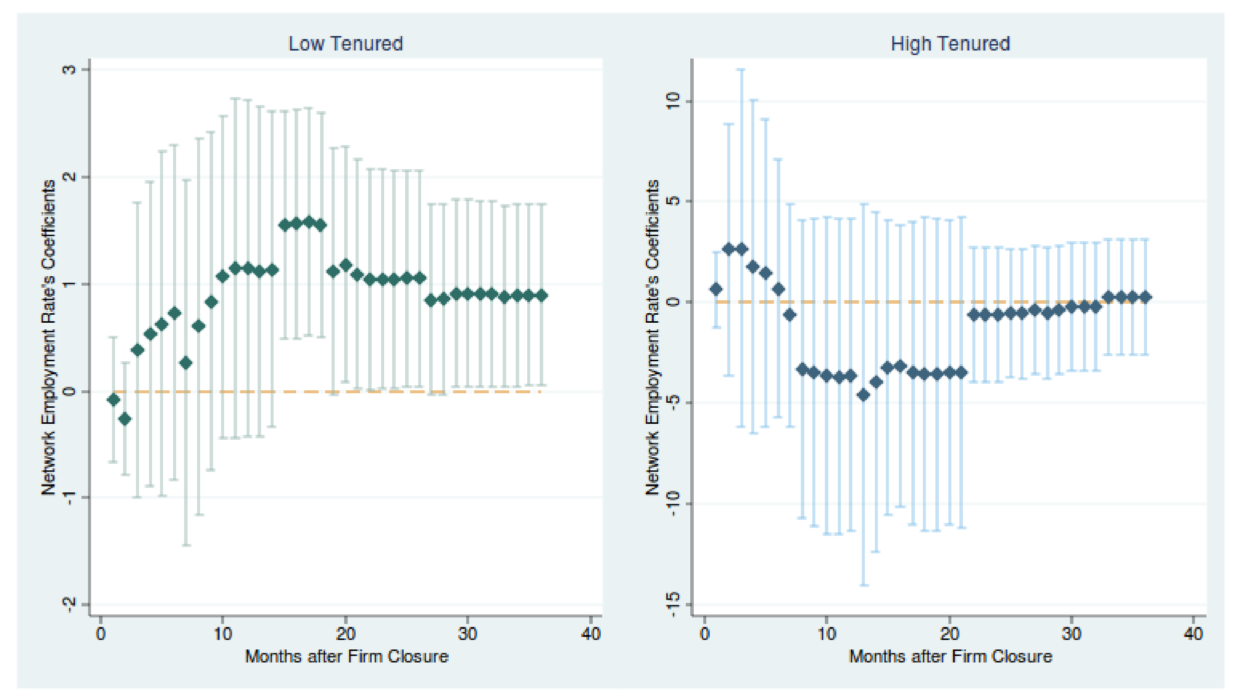
Notes: author's calculations on INPS data for the period 1980- 2001. Closures occurring after December 1998 and before January 1980 are excluded from the analysis. The percentage of censored sample individuals is about 27%. The blue line plots the Kernel density function.

Figure 1.6: Timing of the social effect



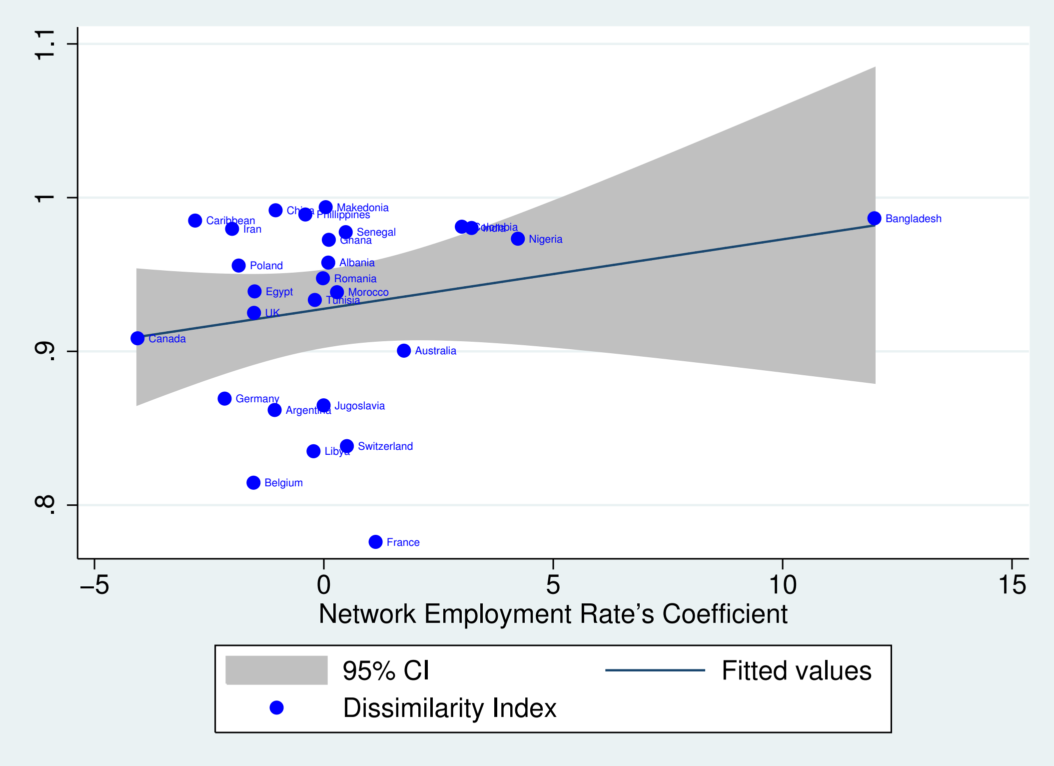
Notes: the coefficients are estimated using equations (1.1) and (1.2), where the dependent variable is the probability of finding a job by each of the 36 months following job loss. Standard errors are clustered by country of origin; controls include age and gender dummies, nationality, time of displacement and the interaction between the first city of work, nationality and time of displacement. The vertical bars in the figure represent the 90% level confidence intervals.

Figure 1.7: Timing of the social effect by tenure in the labour market



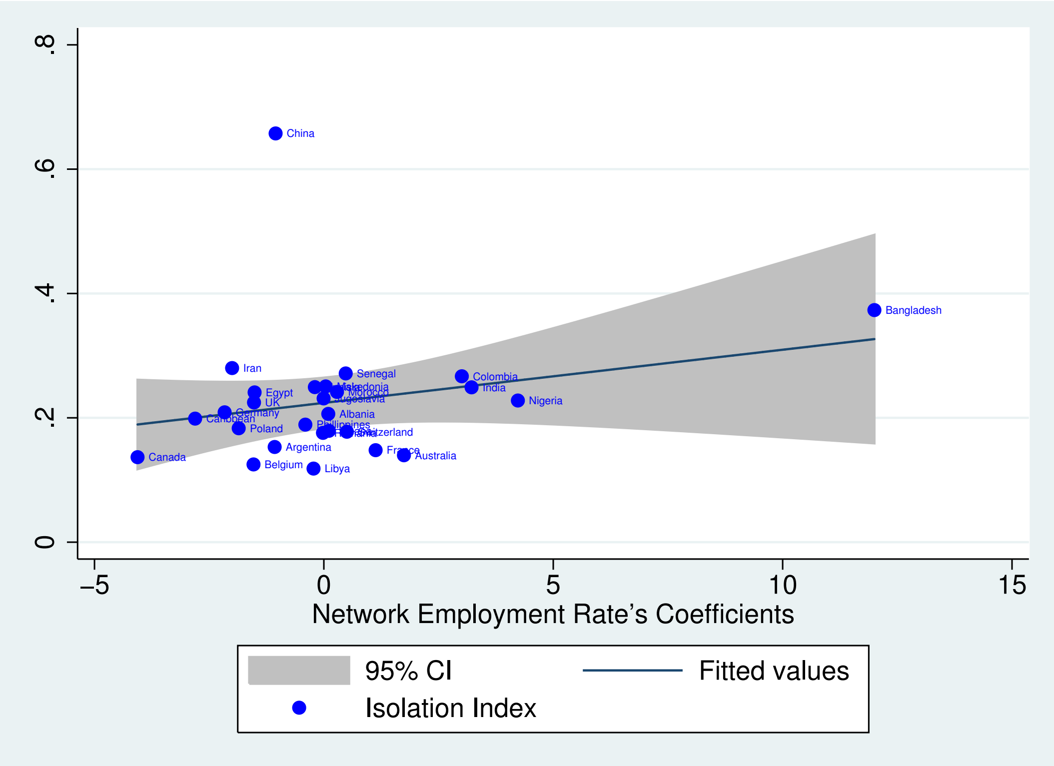
Notes: the coefficients are estimated using equations (1.1) and (1.2), where the dependent variable is the probability of finding a job by each of the 36 months following job loss. Standard errors are clustered by country of origin; controls include age and gender dummies, nationality, time of displacement and the interaction between the first city of work, nationality and time of displacement. A worker is defined as *low tenured* if he has a number of months in employment below the median. The vertical bars in the figure represent the 90% level confidence intervals.

FIGURE 1.8: Social effect and the index of dissimilarity by country of origin



Notes: the coefficients are estimated for each sending country using equations (1.1) and (1.2), where the dependent variable is the probability of finding a job within 36 months following job loss. Standard errors are robust; controls include age and gender dummies, and time of displacement dummies.

FIGURE 1.9: Social effect and the index of isolation by country of origin



Notes: the coefficients are estimated for each sending country using equations (1.1) and (1.2), where the dependent variable is the probability of finding a job within 36 months following job loss. Standard errors are robust; controls include age and gender dummies, and time of displacement dummies.

Table 1.1: Descriptive statistics

	Total	Natives	Migrants
Panel a: All workers			
Number of individual workers	3,604,399	3,339,177	265,222
Number of job matches	12,561,479	11,711,885	849,594
% Workers in the last year of the dataset (2001)	45.53	44.53	58.15
Duration of employment spells (months)	31.16	32.24	16.21
% Male	59.11	58.55	66.17
Age	33.40	33.46	32.06
Gross weekly wage (2003 euros)	683.04	684.27	655.76
Number of co-workers ever worked with	461.07	480.77	213.04
Number of migrant co-workers ever worked with	13.72	12.75	25.93
Occupation:			
% Blue collars	63.16	62.84	71.71
% White collars	29.92	30.19	22.71
% Managers	1.25	1.25	1.08
Transitions (monthly rates):			
Exit rate from employment	1.7	1.65	3.2
Entry rate into employment	1.68	1.62	3.14
Panel b: Displaced workers			
Number of displacement episodes	403,368	385,101	18,267
Number of workers ever displaced	354,073	337,216	16,857
% Workers displaced every month	0.10	0.10	0.14
Characteristics at time of displacement:			
% Male	51.08	50.66	59.88
Age	30.89	30.88	30.99
% Blue collars	67.16	66.82	74.28
% White collars	19.81	19.92	17.55
% Managers	0.27	0.27	0.29
Gross weekly wage (2003 euros)	543.95	545.36	514.26
Probability of having a job after 3 months	49.05	49.17	46.21
Probability of having a job in 4 to 9 months	13.21	13.17	14.15
Probability of not having a job after 9 months	28.95	28.9	29.97

Notes: The table reports averages for the period 1975-2001 based on INPS data. Displaced workers' characteristics refer to the values at the time of displacement.

Table 1.2: Firms and municipalities characteristics

Firms:	
Number of firms	1,121,748
Firm Size	6.87
% Migrant workers	4.26
% Firms in the first year of the dataset (1975)	14.15
% Firms in the last year of the dataset (2001)	24.10
Months in the dataset	142.16
% Firms ever closed	16.32
% Firms closed every month	1.16
Closed firms' size	4.81
Duncan index by migrant status (Firm Level)	0.63
Isolation index by migrant status (Firm Level)	0.27
Municipalities:	
Number of Municipalities	7675
Municipality working population	218.14
Share of Migrants	4.79
Duncan index by migrant status (Municipality Level)	0.25
Isolation index by migrant status (Municipality Level)	0.03

Notes: The table reports summary statistics for the period 1975-2001 based on INPS data.Values for the Duncan and the Isolation indexes are averages across the period 1975-2001.

TABLE 1.3: Probability of re-employment in the 36 months after firm closure - IV Regressions

	All workers			Occupation		Tenure		Country of Origin	
				Blue collars	Others	Low	High	non-OECD	OECD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Network Employment Rate	0.407***	0.318*	0.574**	0.543**	0.283	0.899**	0.258	0.587**	2.551
	(0.134)	(0.168)	(0.266)	(0.267)	(1.789)	(0.433)	(1.463)	(0.253)	(4.100)
First Stage Regressions:									
Network Displacement Rate	0.321***	0.278***	0.533***	0.549***	0.551	0.714***	0.159	0.535***	0.139
	(0.045)	(0.043)	(0.125)	(0.120)	(0.691)	(0.181)	(0.158)	(0.131)	(0.352)
F-Test	51.35	40.74	18.05	42.49	0.63	15.59	1.02	16.60	0.16
Controls:									
Age and Gender Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Network Size Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nationality*Time	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nationality*Time*Municipality	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,738	10,738	10,738	7,635	3,103	5,457	5,281	6,285	4,453

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors in brackets clustered by country of origin; age dummies are defined as: 15-24,25-34,35-44, 45-54, 55-64, 65+; The instrumental variable is the share of network members displaced before the pivotal worker's displacement episode. *Low Tenure* is a dummy variable that takes value equal to one if the pivotal worker has a number of months in employment below the median. *OECD* is a dummy indicating workers whose country of origin was a member of the OECD as of 2001, i.e. Australia, Austria, Belgium, Canada, Czech Republic Denmark, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Korea Luxembourg, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States.

TABLE 1.4: Probability of re-employment in the 36 months after firm closure - Cross group effect and effect on Italians

	Outcome of Displaced Immigrants				Outcome of Displaced Natives	
	Ref. Group: Other Countries		Ref. Group: Italians		Ref. Group: Italians	
	(1)	(2)	(3)	(4)	(5)	(6)
Network Employment Rate	0.048	-0.127	0.032	-0.089	0.095**	0.109**
	(0.129)	(0.282)	(0.062)	(0.098)	(0.039)	(0.045)
First Stage Regressions:						
Network Displacement Rate	0.307***	0.474**	0.577***	0.645***	0.292***	0.255***
	(0.048)	(0.181)	(0.017)	(0.092)	(0.009)	(0.009)
F-Test	40.08	6.83	1147.56	49.56	988.36	906.56
Controls:						
Age and Gender dummies	Yes	Yes	Yes	Yes	Yes	Yes
Network Size Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Nationality*Time	Yes	Yes	Yes	Yes	Yes	Yes
Nationality*Time*Municipality	No	Yes	No	Yes	No	Yes
Observations	10,738	10,738	10,738	10,738	223,936	223,936

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors in brackets clustered by country of origin; age dummies are defined as: 15-24,25-34,35-44, 45-54, 55-64, 65+; The instrumental variable is the share of network members displaced before the pivotal worker's displacement episode. In columns (1) and (2) networks members are past co-workers from other foreign countries of origin; in columns (3) and (4) the reference group is composed of Italian past co-workers. Columns (5) and (6) analyze Italian displaced workers, networks are composed of Italian past co-workers only; for this reason country of origin dummies are automatically dropped in the regressions; standard errors are thus clustered by the date of displacement.

TABLE 1.5: Post displacement outcomes

	Firms		Municipalities		Industries	
	connected	non-connected	connected	non-connected	connected	non-connected
	(1)	(2)	(3)	(4)	(5)	(6)
Network Employment Rate	0.508*	0.066	0.789***	-0.216	0.819*	-0.246
	(0.275)	(0.344)	(0.196)	(0.251)	(0.432)	(0.260)
Controls:						
Age and Gender dummies	Yes	Yes	Yes	Yes	Yes	Yes
Network Size Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Nationality*Time*Municipality	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,738	10,738	10,738	10,738	10,738	10,738

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors in brackets clustered by country of origin; age dummies are defined as: 15-24,25-34,35-44, 45-54, 55-64, 65+; The instrumental variable is the share of group members displaced before the pivotal worker's displacement episode. *Connected* Firms/Municipalities/Industries are dummies equal to one if the displaced worker finds a job in Firms/Municipalities/Industries in which at least one past co-worker from the same country of origin has ever worked before the pivotal individual's displacement episode. *Non-connected* Firms/Municipalities/Industries are dummies equal to one if the displaced worker finds a job in Firms/Municipalities/Industries which no past co-worker from the same country of origin has ever worked.

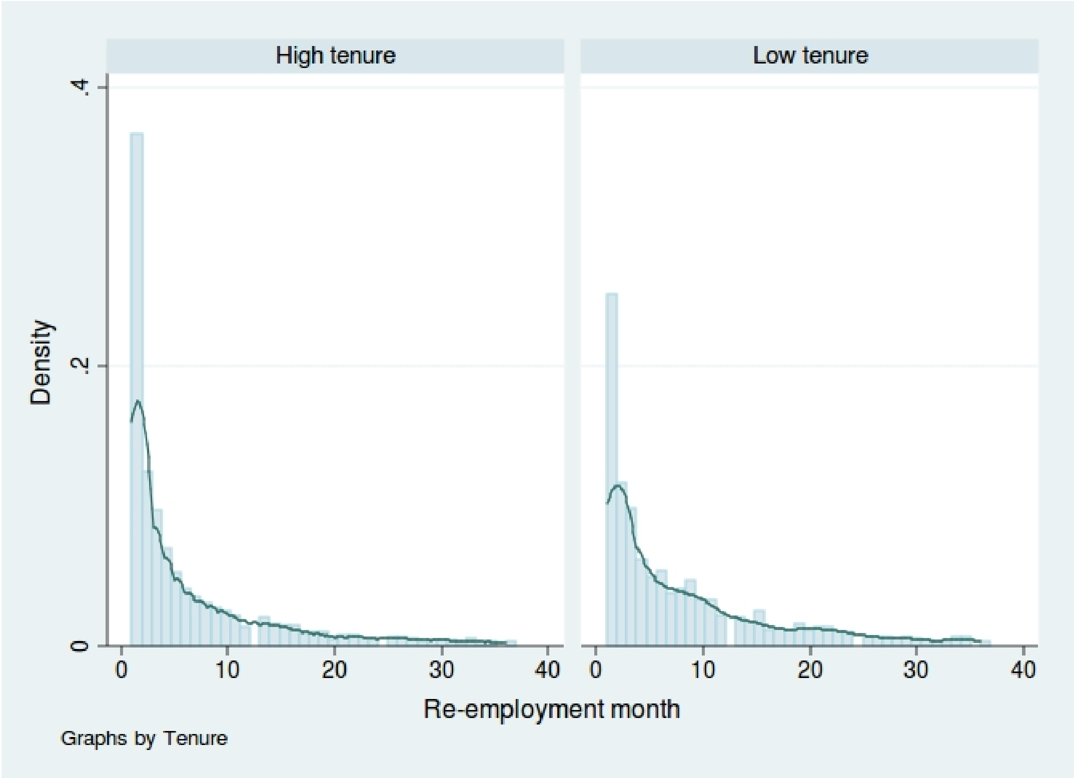
Table 1.6: Network effect and segregation

	Probability of working in 36 months after job loss with			
	Co-national	No co-national	Non-national	No non-national
	(1)	(2)	(3)	(4)
Network Employment Rate	0.779*	-0.205	0.540	0.034
	(0.483)	(0.393)	(0.393)	(0.379)
Controls				
Age and Gender dummies	Yes	Yes	Yes	Yes
Network Size Dummies	Yes	Yes	Yes	Yes
Nationality*Time*Municipality	Yes	Yes	Yes	Yes
Observations	10,738	10,738	10,738	10,738

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors in brackets clustered by country; age dummies are defined as: 15-24,25-34,35-44, 45-54, 55-64, 65+; the instrumental variable is the share of network members displaced before the pivotal worker's displacement episode. Dependent variables are: column (1), the probability of meeting at least one co-worker (new or past) from the same country of origin in the 36 months after the displacement. Column(2), the probability of not meeting any co-workers from the same country of origin. Column (3), the probability of working with at least one co-worker of other foreign nationalities, either past or new co-worker. Column(4), the probability of not meeting any co-workers from a different foreign country of origin.

Appendix A: Supplementary tables

Figure A1: Re-employment probabilities by month (up to 36 months) by tenure



Notes: author's calculations on INPS data for the period 1980- 2001. Closures occurring after December 1998 and before January 1980 are excluded from the analysis. The percentage of the sample individuals censored is about 27%. The blue line plots the Kernel density function.
A worker is defined as *low tenured* if he has a number of months in employment below the median.

Table A1: Networks characteristics

	Mean	Std. Dev.	Min.	Max.
Panel A: Displaced Immigrants				
Re-employment within 36 months	0.669	0.470	0	1
Network's Size:				
Same Country	10.104	37.189	0	228
Other Foreign Countries	5.733	18.098	0	304
Natives	13.967	56.716	0	823
Network Employment Rate:				
Same Country	0.124	0.277	0	1
Other Foreign Countries	0.207	0.329	0	1
Natives	0.209	0.291	0	1
Network Displacement Rate:				
Same Country	0.017	0.101	0	1
Other Foreign Countries	0.032	0.131	0	1
Natives	0.112	0.213	0	1
Panel B: Displaced Natives				
Re-employment within 36 months	0.709	0.454	0	1
Network's Size:	107.413	442.496	0	15,772
Network Employment Rate:	0.380	0.285	0	1
Network Displacement Rate:	0.091	0.138	0	1

Notes: Author's calculations on INPS Data

Table A2: OLS Regressions

	Reference Group: Same Country of Origin			
	(1)	(2)	(3)	(4)
Network Employment Rate	0.205***	0.138***	0.115	0.113
	(0.024)	(0.032)	(0.298)	(0.327)
Controls:				
Age and Gender Dummies	Yes	Yes	Yes	Yes
Network Size Dummies	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes
Nationality*Time	No	Yes	Yes	Yes
Nationality*Time*Municipality	No	No	Yes	Yes
Nationality*Time*Municipality*Industry	No	No	No	Yes
Observations	10,738	10,738	10,738	10,738

Notes: * p<0.10, ** p<0.05, *** p<0.01; standard errors in brackets are clustered by country; age dummies are defined as: 15-24,25-34,35-44, 45-54, 55-64, 65+.

Table A3: Robustness checks

Network Employment Rate:	(1)	(2)	(3)	(4)
Same Country of Origin	0.312*			0.586**
	(0.178)			(0.281)
Other Foreign Country of Origin		0.089		-0.375
		(0.324)		(0.324)
Natives			-0.366	-0.122
			(0.277)	(0.186)
Controls:				
Age and Gender Dummies	Yes	Yes	Yes	Yes
Network Size Dummies	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes
Nationality*Time	Yes	Yes	Yes	Yes
Nationality*Time*Municipality	Yes	Yes	Yes	Yes
Nationality*Time*Municipality*Industry	Yes	Yes	Yes	No
Observations	10,738	10,738	10,738	10,738

Notes: * p<0.10, ** p<0.05, *** p<0.01; standard errors in brackets clustered by country; age dummies are defined as: 15-24,25-34,35-44, 45-54, 55-64, 65+; the instrumental variable is the share of network members displaced before the pivotal worker's displacement episode.

Table A4: Timing of the social effect - IV Regressions

	All workers		Low Tenured		High Tenured	
	(1)	(2)	(3)	(4)	(5)	(6)
Re-employment in:	Coeff.	S.e.	Coeff.	S.e.	Coeff.	S.e.
1 month	0.465	(0.326)	-0.082	(0.298)	0.630	(0.963)
2 months	0.596	(0.371)	-0.260	(0.270)	2.624	(3.164)
3 months	0.824	(0.563)	0.386	(0.703)	2.673	(4.504)
4 months	0.576	(0.494)	0.532	(0.724)	1.745	(4.208)
5 months	0.600	(0.505)	0.630	(0.818)	1.439	(3.879)
6 months	0.535	(0.491)	0.730	(0.798)	0.706	(3.270)
7 months	0.255	(0.555)	0.262	(0.869)	-0.641	(2.801)
8 months	-0.126	(0.532)	0.602	(0.899)	-3.312	(3.759)
9 months	-0.007	(0.616)	0.840	(0.804)	-3.448	(3.887)
10 months	0.102	(0.476)	1.067	(0.768)	-3.642	(3.997)
11 months	0.124	(0.496)	1.148	(0.807)	-3.666	(3.996)
12 months	0.155	(0.505)	1.154	(0.802)	-3.587	(3.939)
13 months	-0.026	(0.477)	1.118	(0.786)	-4.579	(4.800)
14 months	0.048	(0.427)	1.140	(0.749)	-3.944	(4.296)
15 months	0.300	(0.423)	1.551***	(0.541)	-3.238	(3.728)
16 months	0.324	(0.430)	1.563***	(0.547)	-3.140	(3.562)
17 months	0.297	(0.401)	1.582***	(0.543)	-3.485	(3.813)
18 months	0.252	(0.391)	1.550***	(0.536)	-3.562	(3.960)
19 months	0.151	(0.445)	1.124*	(0.588)	-3.558	(3.941)
20 months	0.170	(0.440)	1.180**	(0.562)	-3.463	(3.829)
21 months	0.126	(0.445)	1.093**	(0.546)	-3.467	(3.910)
22 months	0.556*	(0.315)	1.049**	(0.526)	-0.598	(1.702)
23 months	0.531*	(0.316)	1.046**	(0.524)	-0.606	(1.708)
24 months	0.529*	(0.316)	1.048**	(0.519)	-0.606	(1.708)
25 months	0.548*	(0.320)	1.052**	(0.516)	-0.523	(1.625)
26 months	0.548*	(0.319)	1.052**	(0.516)	-0.556	(1.650)
27 months	0.450*	(0.261)	0.856*	(0.452)	-0.377	(1.609)
28 months	0.441*	(0.258)	0.856*	(0.453)	-0.518	(1.672)
29 months	0.474*	(0.269)	0.912**	(0.448)	-0.349	(1.627)
30 months	0.481*	(0.266)	0.915**	(0.444)	-0.230	(1.618)
31 months	0.515*	(0.286)	0.911**	(0.445)	-0.230	(1.618)
32 months	0.515*	(0.286)	0.911**	(0.445)	-0.230	(1.618)
33 months	0.584**	(0.277)	0.885**	(0.433)	0.258	(1.463)
34 months	0.581**	(0.275)	0.894**	(0.434)	0.258	(1.463)
35 months	0.579**	(0.270)	0.898**	(0.432)	0.258	(1.463)
36 months	0.574**	(0.266)	0.899**	(0.433)	0.258	(1.463)
Observations	10,738		5,457		5,281	

Notes: * p<0.10, ** p<0.05, *** p<0.01; standard errors in brackets clustered by country of origin. The coefficients are estimated using equations (1.1) and (1.2), where the dependent variable is the probability of finding a job by each of the 36 months following job loss. Standard errors are clustered by country of origin; controls include age and gender dummies, nationality, time of displacement and the interaction between the first city of work, nationality and time of displacement. *Low Tenured* workers are individuals that have a number of months in employment below the median.

Appendix B: Immigration Policies in Italy

Between 1970 and 1980 Italy changed from being an emigration country into an immigration country; in 1985 the number of foreign residents was almost 500,000, accounting for about 0.8% of the total population. Only in 1986, the first law recognising the legal status to foreigners working and living in Italy was introduced. Few years later, 1990, the Italian government issued a law regulating immigration policy and implementing a quota system; based on the demand for labour of Italian firms, the Italian government had to set every year a maximum number of immigrants that can enter the country.

The main effect of these two first immigration laws was to grant amnesties that conferred legal status to more than 300,000 migrants already working in Italy. The low level of quotas, which were insufficient to satisfy the demand for foreign workforce, and the expectations of future amnesties increased the illegal entry of immigrants. In 1996 and 1998 two other amnesties were granted, regularising respectively 250,000 and 218,000 undocumented foreign workers.

Since 1998, an immigrant who wants to reside and work legally in Italy is required to hold a permit of stay (before this law, legalisation was acquired primarily via amnesties). The permit of stay however does not apply to all migrants: immigrants from countries that signed the Schengen Agreements do not need any permits to live and work in Italy and they can freely enter the country.[27]

The 1998 reform established a maximum period of non-employment following job loss for immigrants to be set equal to one year. In 2001 a new restrictive law passed and the maximum time without working was reduced to six months, past this period, the immigrant becomes unauthorised and he/she has to leave Italy. In the same year the biggest amnesty took place regularising almost 650,000 undocumented foreign residents.

[27]Moreover countries belonging to the European Union are excluded. In the observation period (i.e. up to 2001), migrants from the following countries were exempted from the permit of stay regulation: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Luxembourg, Netherlands, Portugal, Spain, Sweden, United Kingdom.

Overall Italian immigration amnesties involved almost 1.5 million individuals: it is clear that amnesties represented the main gateway into the country. In order to be eligible for regularisation a migrant has to show a regular job offer.

The estimates on illegal migrants are based on the number of applications to amnesties, these measures are very noisy and range from 10 to 40 per cent of the legal workers, i.e. in 2001 1.4 million of migrants were present in Italy meaning that the estimates of illegal migrants are around 140,000 to 500,000 unauthorised migrant. (Venturini and Villosio,2008, Fasani 2010). Several institutions, such as *Caritas* of the national statistics office, ISTAT, also provide estimates of illegal migrants operating in the black economy.

Chapter 2

Social Ties in Academia: a Friend is a Treasure

2.1 Introduction

The degree of institutional and geographical concentration of authors and editors of top economics journals has always been largely biased towards the United States (Ellison 2002; Kim, Morse and Zingales 2006). In 1995 about 71% of the editors of the 30 most cited journals came from an institution located in the United States and thirteen U.S. universities accommodated about 39% of all editors. Similarly, 65.7% of journal articles' authors were located in a U.S. institution and the same thirteen universities accounted for 21.8% of the authors (Hodgson and Rothman 1999).[1] Goyal, van der Leij and Moraga-Gonzalez (2006) also show that the world of economists who publish in high-impact journals is small and integrated: almost half of this population is composed of interconnected authors, whose average distance, measured by degrees of academic separation, is relatively short.[2]

Among different potential explanations for these figures, one is given by *sorting* of individuals: most talented researchers tend to locate in the same top universities, eventually increasing the gap in academic productivity between elite and non-elite institutions (Kim, Morse and Zingales 2009). A second explanation has

[1]The thirteen U.S. universities are Harvard, Chicago, U. Pennsylvania, Stanford, Northwestern, U .Wisconsin, U. Cal. Berkeley, U. Michigan, MIT, Princeton, Yale, NYU, and U. Maryland.

[2]Suppose that every author is a node in a network, the distance between two nodes is equal to one if they have co-authored at least one paper; it becomes equal to two if the two scholars have never worked together but they share a co-author, and so on.

to do with *peer effects* among researchers (Waldinger 2012; Borjas and Doran 2012). The collaboration and the exchange of ideas with prominent colleagues are valuable mechanisms to foster scientific knowledge, therefore the agglomeration of highly productive scholars in a particular university may generate positive spillovers on the scientific production of its members (Azoulay, Graff Zivin and Wang 2010; Waldinger 2010).[3] Finally, *editorial favouritism* (Laband and Piette 1994; Brogaard, Engelberg and Parsons 2013), i.e. editors' practice of favouring in the publication process professionally linked scholars, is a third potential explanation. This paper investigates this issue by estimating to what extent connections between authors and editors affect the publication process in economics and ultimately the quality of published articles.[4]

To this end, I employ a unique dataset providing detailed information on academic histories of authors and editors of the top four economics journals over the period 2000-2006. I assembled a new dataset on all scholars that published at least one article, or served as editors, in the top four general interest economics journals, i.e. *the American Economic Review*, *the Journal of Political Economy*, *Econometrica* and *the Quarterly Journal of Economics*, from 2000 to 2006. The data provide yearly information on authors' and editors' academic careers since the time they graduated up to the last year of the observation period, i.e. 2006. This dataset allows me to identify whether social ties between each author and each editor actually exist along various dimension; it is thus possible to explore what determinants of network formation are the most relevant in academia. I focus on five different types of connections: I define an author and an editor to be connected if they are employed by the same institution in the year before the editor started his appointment (*same faculty*); if they have ever worked in the same institution (*faculty ever*); if they received their PhD from the same university in the same time period (*same PhD*); if the editor was one of the PhD advisors of the author (*PhD advisor*); and if the author has ever co-authored at least one paper with the editor in the past (*co-authors*). The dataset contains information on 1,621 journal articles written by 1,828 scholars; articles' characteristics include the number of

[3]More precisely, Waldinger (2010) uses the expulsion of Jewish scientists in German universities during the Nazi era as an exogenous variation in the quality of universities. He shows that graduate students enrolled in institutions that experienced the larger drop in the average quality also experienced a decrease in the probability of publication and in the probability of getting tenured. Azoulay Graff Zivin and Wang (2010) focus instead on the effect of *superstar* scientists on the productivity of their co-authors, showing a long lasting decline on their publication rates when the superstar they are linked to dies unexpectedly.

[4]Social ties in academia, as well as in many other contexts, are believed to be a key driver for a successful career (Combes, Linnemer and Visser 2008; Ioannides and Datcher Loury 2004).

citations, the number of pages, the position within the journal issue and the topic according to the *JEL* classification.

Measuring the causal effect of connections with an editor on a scholar's publication probability requires to tackle different empirical challenges. First, *endogenous group formation* represents a threat to identification if scholars who tend to be connected to editors are systematically different (Soetevent 2006). As editors are usually selected among highly reputed scholars, connected authors are likely to be equally skilled; a positive correlation between an author's probability of having his paper published and the existence of a link with the editor may be simply driven by unobserved characteristics, such as ability. Second, scholars in the same networks may share the same characteristics, such as the field of research, and they are thus likely to be exposed to similar shocks in publication trends; this case of *correlated effects* may induce a positive correlation between a scholar's editorship and the publication probabilities of his connections (Moffit 2001).

In order to identify the effect of connections on the publication probability, I exploit editors' rotations, i.e. differences in the publication outcomes of authors connected to a senior scholar when this scholar is in charge as an editor and when he is not.

Regression results show that the existence of a social tie with an editor positively affects publication outcomes: when a scholar is in charge of a journal, the number of papers published by authors connected to this scholar increases by about two papers in three years. In particular, editors tend to publish papers of scholars working in the same institution and of former PhD students: for these types of connections the number of articles published increases by 1.32 and 3.54 papers in three years. Current colleagues of an editor are also more likely to publish lead and longer articles during that editor's appointment. Being a past co-author of an editor does not have an effect on the publication rate, but it does affect the length of an article and its position within the journal issue.

In general, there are two competing arguments to explain why a preferential treatment of editors towards connected scholars arises. On the one hand, professional links may increase the quality of a paper through *technological complementarities.* The quality of a paper depends on the author's and the editor's input. Pre-existing ties between scholars might reduce the cost of communication and increase cooperation, improving the quality of a paper. Over the last twenty years, the number of total submissions to top economics journals has almost doubled, while the number of published articles has decreased (Card and Della Vigna 2013). It has become extremely costly for editors to screen the large amount of submissions received. As editors want to publish the best papers, the publication probability of connected scholars may then increase.

On the other hand, editors may favour connected authors because of *tastes*, as they share a common view on what should matter to economics or because of *nepotism* (Bagues and Zinovyeva 2013; Durante, Labartino and Perotti 2011). If this is the case, the publication standards applied by the editor to connected papers may be lower than for non-connected ones, possibly resulting in an impoverishment in the quality of publications. Although these two stories lead to the same implication on the publication probability of connected authors, they predict an opposite sign on the quality of published papers.

The analysis of subsequent citations received by articles can shed some light on the effect of social connections on the quality of publications also highlighting the main mechanisms at work. Empirical estimates show that among articles published in top economics journals, the ones authored by connected researchers have a higher citation rate compared to non-connected ones. For instance, articles authored by university colleagues of an editor experience an increase in citations of about 8.6 quotes when this editor is in charge. These results suggest that technological complementarities between connected scholars working in the same institution exist, improving the quality of *connected* papers. Unexpectedly, there is no statistically significant effect for other types of connections on the number of citations, possibly implying that the positive effects on quality due to technological complementarities are offset by a dilution in quality due to nepotism.

The remainder of the paper is organised as follows. Section 2.2 presents a simple model to show how connections influence editors' behaviour. I provide details on the data collection and key summary statistics in Section 2.3. Section 2.4 describes the empirical strategy. I present and discuss the empirical results in Section 2.5. Section 2.6 concludes.

2.2 Conceptual Framework

This section provides a theoretical framework to illustrate how editors' choices in the publishing process are influenced by the existence of a social tie with authors, and how these connections ultimately affect the quality of published articles. Social ties may affect editors' behaviour through two channels: tastes and technology.

When an author and an editor are professionally linked, the cooperation and the communication between them are likely to be higher than between non-connected scholars. For instance, an editor can more easily review a colleague's paper if he attended the paper's presentation at an internal seminar; or if he can benefit from comments of other researchers working in the same institution. The cooperation between connected scholars then may increase the quality of a paper through technological complementarities. Social ties may also directly enter editors' preferences: either because of altruism or because of a taste for power (Rotemberg1994; Pendergast and Topel 1996; Bandiera, Barankay and Rasul 2009).[5]

In order to formalise these mechanisms and explore their effects on publication outcomes, assume that a journal editor receives a *connected* and a *non-connected* paper to review for publication. The final quality of every submitted paper is given by $y_c = \alpha_c + k_c$; where c indicates whether the submitted paper is connected ($c = 1$) or non-connected ($c = 0$).[6] The quality of a paper depends on the ability of the author α_c, which for simplicity I assume being normally distributed with mean μ and variance equal to one, $\alpha_c \sim N(\mu; 1)$. Social ties do not affect the distribution of authors' ability, i.e. $\mu_0 = \mu_1 = \mu$; however they can directly affect the quality of submitted papers through the parameter k_c, which captures the increase in papers' quality due to technological complementarities between authors and editors. For simplicity I assume that this parameter is equal to zero ($k_0 = 0$) for non-connected scholars, and it is positive ($k_1 = k > 0$) when there is a tie between the editor and the author.

Editors have to chose which article to publish in order to maximise their payoff, this is:

$$\max_D y_0(1 - D) + (y_1 + g)D \; , \tag{2.1}$$

where D is a binary variable that takes value one if the connected paper is published and g is the editor's private return from publishing the work of a connected author.

From the solution of editors' problem, the probability of publishing a connected paper is:

$$Pr[D = 1] = Pr[\alpha_1 - \alpha_0 + (g + k) > 0]. \tag{2.2}$$

[5]The model of Bandiera, Barankay and Rasul (2009) shows that social connections between workers and managers in the firm increase the level of managerial effort while they have an ambiguous effect on the firm's overall performance.

[6]For simplicity I do not model effort choices of both authors and editors in the publication process.

Assuming that α_1 and α_0 are independently distributed, the probability that an editor will publish the work of a connected author is always greater than one half, i.e $Pr[D = 1] > 1/2$.

The first implication of the model is that editors will publish connected papers as long as technological complementarities and/or private returns from publishing the work of a "friend" exist.

From the above equations, it follows that the expected quality of connected published papers is:

$$E[y|D = 1] = E[y_1|\alpha_1 + g + k - \alpha_0 > 0] = k + \mu + \sigma\frac{\phi(z)}{1 - \Phi(z)} , \tag{2.3}$$

where $z = \alpha_1 - \alpha_0 + (g+k)$, $\frac{\phi(z)}{1-\Phi(z)}$ is the inverse Mills ratio, and σ is the standard deviation of z. $\phi(.)$ and $\Phi(.)$ are the pdf and the cdf of the standard normal distribution, where $\Phi(z)$ is the probability of publishing a non-connected paper, i.e. $Pr[D = 0]$.[7]

Following the same reasoning, the expected quality of a published non-connected paper is:

$$E[y|D = 0] = E[y_0|\alpha_0 - (\alpha_1 + g + k) > 0] = \mu + \sigma\frac{\phi(z)}{\Phi(z)} \tag{2.4}$$

By subtracting equation (2.4) from equation (2.3), the difference in expected quality between connected and non-connected papers is:

$$E[y_1|D = 1] - E[y_0|D = 0] = k + \sigma\frac{\phi(z)[2\Phi(z) - 1]}{\Phi(z)[1 - \Phi(z)]} \tag{2.5}$$

The first term in equation (2.5) is the technological parameter, which is positive by assumption ($k > 0$). However from equation (2.1) it follows that the second term of the equation 2.5 is always negative, as $Pr[D = 1] > 1/2$.

The second implication of this theoretical framework is that the differential quality between connected and non-connected papers has an ambiguous sign: technological complementarities increase the quality of connected publications; however because of tastes editors may decide to publish connected papers of a lower

[7]Under the assumption that α_1 and α_0 are two independent normally distributed variables, it follows that $z \sim N(0; \sigma^2)$ where $\sigma^2 = 2$ and the correlation coefficient between α_1 and z is equal to one.

quality with respect to non-connected ones. These results constitute the basis of the empirical strategy of this paper.

2.3 Data and Measures of Connections

The data used in this work provide detailed information on all articles published in the *American Economic Review* (AER), *the Journal of Political Economy* (JPE), *Econometrica* (ECA) and *the Quarterly Journal of Economics* (QJE), i.e. the leading American general economics journals, over the period 2000-2006. I exclude from the sample papers published in the Annual Papers and Proceedings issues of the American Economic Review, as well as announcements, comments, replies or notes.

The main data sources for the articles are *IDEAS-RePEc* and *Web of Science*.[8] For each article the data report the number of citations received as of December 2012, authors' first and last name, the date and the issue of publication, the number of pages and the field according to the *JEL* classification (one digit).

Starting from these data, I collected and skimmed each author's *curriculum vitae* to construct a longitudinal dataset that allows me to follow any scholar in every year since the time they graduated until the last year of observation, i.e. 2006;[9] I have information on authors' gender, country of origin, university and year of award of the PhD, institution in which they are appointed and their position within the institution in every year since graduation, editorial activities and main research fields classified according to the JEL classification.[10] There are 1,828 authors attached to the articles analysed.

The same information is provided for the 42 scholars that have served as editors for at least one issue in the four journals analysed over the period 2000-2006. The

[8] Research Papers in Economics (RePEc) is a collaborative effort of hundreds of volunteers in 76 countries to enhance the dissemination of research in economics. RePEc provides links to over1.4 million research pieces from 1,700 journals and 3,700 working paper series (http://repec.org/).
Web of Science (WoS) is a scientific citation indexing service that provides a comprehensive citation search. It gives access to about 30,000 scholarly books, 12,000 journals and 148,000 conference proceedings, representing one of the largest citation databases (http://thomsonreuters.com/web-of-science/).

[9] About 95% of the authors in the sample have their CV publicly available on the web; when this was not provided, I gathered information from alternative sources such as *Wikipedia* or the departmental web pages. Missing authors account for less than 3% of all authors.

[10] Appendix A provides a detailed description of the JEL classification system.

names of the editors were retrieved from journals' archives and from *JSTOR*.[11] I further recovered from editors' *curricula* the names of their co-authors up to 2006.

Table 2.1 reports summary statistics for the articles in the sample. Overall, there are 1,621 articles published in the top four economics journals over 7 years. The American Economic Review is the journal that published the largest number of articles (604), while the Quarterly Journal of Economics the smallest (282). The latter tends to publish longer papers, with an average number of pages equal to 36.6.[12]

Statistics show that the QJE is the most cited journal, while ECA ranks last (54.10 vs. 25.79). Figure 2.1 further explores data on citations, it plots the cumulative distribution of papers' citations by journal: the QJE is also the journal with the smallest share of low-cited articles, i.e. articles with less than 10 citations, and the highest share of highly-cited ones, i.e. articles with more than 200 citations. This can be seen by the fact that the QJE's cumulative distribution line is flatter than the one of other journals.[13]

The number of citations is likely to depend on both the length and the field of the article, possibly explaining differences in citations across journals. For instance, ECA mainly publishes econometric and theoretical papers that, in recent years, have been less cited than applied ones (Card and Della Vigna 2013; Hamermesh 2012). On average there are about 2 authors per article, ECA tends to publish papers written by fewer authors while the average number of authors that publish in the QJE is the highest (2.03), potentially explaining differences in citations between these two journals. Moreover, if I restrict the sample to single authored papers, the average number of citations decreases and the differences in citations across journals become smaller. Table 2.2 provides OLS estimates of the effect of different articles' characteristics on the number of citations received by each article.[14] Column (1) shows that citations are positively correlated with the number

[11] I did not consider potential hierarchies among the editors; for instance, I listed as "editors" both *editors-in chief* and *co-editors*.

[12] However this value strictly depends on the formatting used by the journal: as of 2006 the AER was the only journal using the two-column format, explaining the lower length of its articles. The AER eventually switched the one-column format in 2008. This journal also publishes in every issue a set of *shorter papers* that generally do not exceed the 15-page length.

[13] Data on citations indicate the number of papers that have cited the article since the release date up to December 2012, including working papers and self citations. Figure B.1 plots the distribution of citations and its kernel density by journal, as we can see the distributions are heavily skewed to the left.

[14] I estimate the following equation:$y_{ijt} = \alpha_0 + \mathbf{X'}_{ijt}\alpha_1 + \lambda_i + \theta_j + \eta_t + \epsilon_{ijt}$; where y is the number of cumulative citations of article i published in journal j in year t. $\mathbf{X}$ is a set of articles characteristics, including authors dummies, number of pages and JEL codes.

of authors; for instance, articles authored by four scholars receive 32 more citations than single-authored papers. This result is robust to the inclusion of other articles' characteristics and it only slightly decreases when controls for the journal issue are included. As expected, econometric papers receive 26 less citations than papers in other fields (see column (2)), while longer articles have a higher number of citations: one additional page increases citations by about one, however this relationship becomes weaker as the number of pages increases (Column (3)).

2.3.1 Definition of Social Ties

Table 2.1 also reports the fraction of papers by authors that were connected with at least one editor at the time of the publication. As mentioned in the introduction, I focus on five different types of social ties. The first one, which I define as *Same PhD*, indicates if an editor and a scholar obtained the PhD from the same university in the same time window (allowing for a maximum 3 year gap between graduation dates). Overall less than 9% of the 1,621 articles published in the period 2000-2006 were written by authors connected because of the university and year of PhD award.

A *PhD Advisor* connection is established when an editor had an academic position in the same university and in the same year in which the author obtained his PhD and the two scholars also share the same research field. Since I do not have information on the PhD advisors for all the authors, this variable proxies for the link between academic advisors and their students. The fraction of connected papers according to this second measure is about 15%, with the QJE showing the highest figure, i.e. 22%.

I then investigate the role of connections between colleagues; according to this definition two scholars are socially tied if they work in the same institution in the year before the editor becomes in charge of the journal, i.e. *Same Faculty*. I further classify two scholars as connected if they have ever been employed in the same faculty at any point in time before one of the two was appointed as editor, i.e. *Faculty Ever*. On average, the QJE has the highest share of connected authors according to this last metric, implying that about one fifth of the articles published in the QJE over the period 2000-2006 were written by scholars affiliated with Harvard. The Quarterly Journal of Economics and the Journal of Political Economy are "house journals", i.e. they have at least one editor coming from the university in which they are hosted, which are Harvard University and the University of Chicago respectively.

Finally I examine social ties based on co-authorships; I define an editor and an author as *Co-authors* if they have ever co-authored at least one paper, either published in a journal or a working paper, up to the year in which the editor started his appointment. The share of *Co-authors* connected papers is around 11%, journals publishing more papers of editors' co-authors are ECA and the QJE, whose shares are 12.82% and 17.73% respectively. Overall, about 43% of papers published in the four journals considered are authored by at least one scholar that is connected to at least one editor at the time of the release.

The degree of concentration of this particular "market" can be analysed by computing the *Herfindahl index* (HHI) of institutions' "market shares" of articles in the top four general-interest journals over the observation period (Ellison 2002). This is the sum of the square of the market share of the largest 50 universities in terms of academic production observed in the data. The market share for each institution is computed as the number of authors employed by the institution at the time of the publication over the total number of authors that have published in the journal. As shown in the last row of Table 2.1 and consistent with Figure 2.2, the QJE is the journal with the highest concentration index, i.e. about 5%. Relatively high values of the HHI index are also found for the JPE, while the AER and ECA show the lowest values of concentration.[15][16]

Figure 2.2 plots the distribution of authors who published in any of the four journals considered according to their institution. ECA and the AER seem to be more open than the QJE and JPE, which show a bias towards authors appointed at their host institutions. Roughly 10% of the authors of papers appearing in the JPE were employed by the University of Chicago at the time of the publication. Similarly, only seven universities contributed to 50% of the articles published by the QJE in the seven years considered, with Harvard alone accounting for about 15%. Figure 2.3 provides even more striking results, it plots the distribution of authors according to the institution of PhD award. As in Figure 2.2, top U.S.

[15]It is hard to comment on the absolute levels of concentration, especially because this market is not comparable to other standard markets; in general, values of the HHI index greater than 0.15 are considered high, implying that the market is an oligopoly with a medium-high level of concentration.

[16]The Herfindahl index for journal j is defined as $HHI_j = \sum_i s_{ij}^2$, where s_{ij} is the fraction of all articles in journal j written by authors affiliated at the institution i. In Appendix B, Figure B.2 shows the evolution of the HHI index by year and journal.

universities are overrepresented; for instance, Harvard and MIT alone account for about 50% of all papers published in the QJE.

Table 2.3 provides a more detailed picture of the characteristics of the editors and authors at the time of publication. Out of 1,828 authors, about 40% were born in the U.S., the second largest group is represented by Italian born economists. The data do not provide the year of birth, however the date of graduation proxies for the "academic age": less than one author out of four is an early career, i.e. he/she got the PhD at most four years before the publication. Experienced and male scholars are the most represented groups in the sample, with a share of full professors is roughly 52% and a share of male economist close to 90%.

Among the authors in the sample, about 29% published at least two articles; the most prolific economist over this period is John A. List with 14 articles published in 7 years, 5 in the QJE, 4 in the AER, 3 in the JPE and 2 in ECA. Figure B.3 in Appendix B shows the list of authors who published more than 4 articles from 2000 to 2006.[17]

Finally, it is quite interesting to observe that while about 20% of the authors received their PhD from MIT or Harvard, for editors the share increases to 40%. Editors are usually American male professors, most of them come from the universities of Chicago and Harvard; the share of scholars who are connected to at least one editor of any journal at any time is very high: about half the authors were working in an institution in which at least one editor also worked in the past; moreover, 40% of authors were supervised by a scholar who was or became editor in one of the top 4 economics journals.

The idea of economics being a small world seems to be confirmed by these statistics. Whether connections have a causal impact on the probability of publishing and on the quality of the publications is thus a topic worth investigating.

2.4 Identification

The aim of this section is to estimate the causal effect of connections between scholars and editors on two outcome variables of the publication process: the probability of getting a paper published and the number of citations the paper receives.

[17] Figure B.4. reports the most productive authors by journal.

The main empirical challenge is that connected authors have a higher probability of publication in top economics journals for reasons other than the existence of a connection with the editor. In order to address this challenge, my empirical strategy compares publication outcomes of the same connected authors when a connected editor is in charge of a journal and when he is not. For each editor i in charge of journal j at time t, I identify papers published by connected authors in journal j at any time over the observation period 2000-2006.[18]

For instance, David Card was a co-editor of the AER from 2002 to 2005. To him I assign all articles published in the AER in the time span 2000-2006; I then identify articles authored by current and past university colleagues, former PhD students, co-authors, and PhD colleagues. I then estimate whether the number of articles connected to Card and published by the AER changes depending on Card's editorship, based on the following linear regression model:

$$y_{ijt} = \beta_0 + \beta_1 InCharge_{ijt} + \lambda_i + \zeta_{jt} + u_{ijt} \ , \tag{2.6}$$

where y_{ijt} is the number of papers connected to editor i that have been published in journal j at time t, $InCharge$ is the treatment variable, which takes value one whenever editor i is in charge of journal j at time t, and zero otherwise. The coefficient of interest is β_1, which indicates to what extent an appointed editor affects the publication outcomes of connected scholars in journal j. Finally λ_i and ζ_{jt} are editor and journal*time fixed effects respectively. Since every journal has different release dates, the time variable is journal specific; in order to circumvent this I aggregate issues by semester.[19]

The identification of model 2.6 is based on the comparison of publication outcomes of connected scholars when editor j is in charge and when he is not, like in a classical event study analysis (Lee and Mas, 2012). Editor fixed effects control for the unobserved quality of the network. The ζ_{jt} control absorbs any trend in publication outcomes in journal j, including shifts toward particular research fields that can influence the publication outcomes of connected authors and the appointment of the editor.

[18] If a scholar is appointed as an editor of two different journals I treat him/her as two independent observations. Given the short time window, i.e. seven years, the data just have one scholar, i.e. Nancy Stokey, who was editor of two different journals, which are Econometrica first and the Journal of Political Economy afterwards.

[19] The JPE and ECA publish six issues per year, while the AER and the QJE only have four issues per year. ECA release dates are January, March, May, July, September and November; for the JPE these are February, April, June, August, October, December; the AER publishes in March, June, September and December. The QJE releases its issues in February, May, August and November.

The final specification is then a differences-in-differences model, where journal-time characteristics and editor fixed effects are fully absorbed. The identification of parameter β_1 comes from changes in the composition of editors within the journal j. The average duration of the appointment is 3 years, (or 16 issues). In order to identify the parameter of interest, β_1 there must be at least one new editor appointed in each journal over the observation period.

The definition of the dependent variable y_{ijt} changes depending on the outcome of the publication process considered; for instance, I focus on connected authors' number of published papers, number of pages, and number of lead articles in journal j at any time t.[20] The dependent variable also changes according to the type of social tie analysed; for instance, an outcome variable is the number of published papers written by former PhD students of editor i, or the number of published papers written by university of editor i, and so on.

Finally and in line with the theoretical model, to estimate the causal effect of connections on the quality of the papers, I define as a dependent variable the average number of citations that connected papers receive.

2.5 Empirical Findings

2.5.1 Effects of connections on the publication probability

Table 2.4 reports estimates of equation (2.6). The single observation is represented by the combination of editor i, journal j and semester t; for instance, at any time t an observation in the sample is the number of papers published in the AER authored by scholars connected to David Card. Since I have aggregated observations by semester, the independent variable $InCharge_{ijt}$ is defined as the fraction of journal issues the editor was in charge of, over the total number of journal issues in semester t.[21] The total number of observations is equal to 602. All the regressions presented in this section include editor and time*journal fixed

[20] Whenever an article has more than one author, I define it as connected if at least one author has a tie with the editor.

[21] Since journals have different release dates of their issues, the inclusion of a time fixed effects in equation (4) would not be independent of the journal. In order to control for time trends, I need to aggregate the data by term so that the time variable becomes biannual, being no longer journal-specific.

effects. Standard errors are clustered by the interaction between journal j and time t.[22]

Results in Panel A of Table 2.4 show that the number of published articles connected to an editor increases when this editor is in charge. In column (1) authors and editors are considered as connected if at least one of the five social ties exists. The coefficient is positive and significant, it implies that when the editor is in charge of a journal the number of published articles connected to an editor of that journal increases by 0.31 papers per semester. Since the average duration of an editorship is about three years, the increase in the number of connected papers published during an editor's appointment is approximately 1.86.

Columns (2) to (6) investigate what type of professional link drives the positive coefficient in column (1): the only statistically significant coefficients are the ones related to the *same faculty* and the *PhD advisor* ties. The number of articles written by authors who share the same institution as the editor raises by about 0.59 articles per semester when the editor is in charge. In other words, authors increase their publication rate by more than three papers in a journal when a connected scholar is appointed as the editor of that journal. A lower but still positive and significant effect is found for the *PhD advisor* connection, the coefficient implies that following the PhD advisor's appointment as an editor of a journal, the number of publications of his former PhD students increases by about 0.22 articles per semester. All other types of social ties appear to have smaller and statistically insignificant effects on the publication outcomes.

The data used in this work also provide information on other characteristics of the articles, such as the number of pages and the position within the issue, i.e. whether a paper is the lead article. Coupé, Ginsburgh and Noury (2010) use a natural experiment to show that leading papers in randomly ordered issues attract more citations. It is then worth investigating whether connections also have an effect on these two outcomes of the publication process. Panel B of Table 2.4, presents results of the effect of social ties on the number of pages of articles written by connected scholars (number of connected pages). Connected authors experience an increase in the number of pages published by about 15 pages per term when one of their connections becomes editor. The effect again is positive and significant for the *same faculty* and *PhD advisor* links, with estimated coefficients of 18.7 and

[22] Standard errors are not clustered by editor as the number of clusters in this case would be low, i.e. 42. When clusters are few, serial correlation may be underestimated and the estimated coefficients potentially biased (Angrist and Pischke 2008). In this case, regressions in which errors are clustered at the editor level produce smaller standard errors.

6.4 respectively; the number of pages per article raises by about twenty-nine pages per term when the author is a colleague of the editor at the time of publication; while they increase by slightly more than six pages per semester when the author has been a graduate student of the editor. Again, the *same PhD* link does not provide significant results, however the effects of the *faculty ever* and *co-author* connections turn out to be positive and significant.

Panel C further provides results on the probability of having a connected paper published as the lead article of the issue. The coefficients are positive for the links based on the institution of appointment, i.e. *same faculty*, and for the one based on past co-authorships, *co-author*, while the *Phd advisor* is no longer significant.

In the previous regressions, a paper is defined as connected if at least one author is linked to the editor; papers can be authored by more than one connected author, I therefore estimate whether the number of authors who are socially tied to the editor increases during this editor's appointment. The effect remains positive and significant in column (1) implying that, when the editor is in charge, the number of connected authors who publish increases by 0.49 authors per term. The effect is positive and significant for the *same faculty* and *PhD advisor* ties, indicating that the number of colleagues of the editor who publish in one of the top economics journals increases by about 0.66 and 0.23 per term respectively. The estimated coefficients for the other ties are not statistically significant.

As predicted by the theoretical model in Section 2.2, the empirical findings presented in Table 2.4 confirm that editors tend to publish papers of authors they are connected to: the publication probability, the page length of articles and the position within the issue of connected scholars increase when editors start their appointment. These estimates also indicate that the positive effect is particularly relevant for some types of connections: PhD students and university colleagues. According to the implications of the theoretical model, positive technological complementarities and/or private returns from publishing a "friend" only exist for these two types of connected authors.

2.5.2 Robustness checks

The empirical model developed in Section 2.4 implicitly assumes that when an editor finishes his mandate, the publication probability of his connections reverses to the one in pre-editorial terms. This model is univocally restrictive as the state dependence in the publication process may imply that the effects of a connection with a scholar are persistent, even when the editor is no longer in charge. I order to investigate this issue further, I estimate the following equation:

$$y_{ijt} = \gamma_0 + \gamma_1 InCharge_{ijt}^{entry} + \gamma_2 InCharge_{ijt}^{exit} + \zeta_{jt} + \lambda_i + u_{ijt} \tag{2.7}$$

where $InCharge_{ijt}^{entry}$ is a dummy that is equal to one at any time following editor j's appointment. For instance, for David Card this variable is one at any time after January 2003. The variable $InCharge_{ijt}^{exit}$ is another dummy that is one at any time t after editor j finishes his mandate. By subtracting the *exit* dummy from the *entry* dummy, I recover the $InCharge$ variable of equation (2.6), i.e. $InCharge_{ijt}^{entry} - InCharge_{ijt}^{exit} = InCharge_{ijt}$. The coefficient γ_1 then measures the persistence of the editorship's effect, while γ_2 measures the effect of an editor loosing the editorship on connected authors' publication outcomes.[23] Finally ζ_{jt} and λ_i are journal*time and editor fixed effects respectively.

Interestingly results reported in Table 2.5 show that the effects of a connection on publication outcomes are highly persistent. Moreover, these effects do not vanish when the editor terminates his appointment. For instance, the number of articles written by former PhD students increase by 0.36 per semester; however, former PhD students seem not to experience a decrease in publication outcomes when the editorship ends, as the coefficient of the $InCharge^{exit}$ is statistically insignificant. On the contrary, the number of articles and the number of pages published by editor's faculty colleagues decrease by 0.27 and 6.07 per term respectively when the editor is no longer in charge.

Clearly one may interpret the the persistent effects found in Table 2.5 as evidence of connections' state dependence; however, I cannot rule out that they may also reflect a delayed effect caused by the time gap between a paper's submission and its publication, i.e. the review times of articles, which usually spans from one to two years (Ellison 2002).[24]

An additional concern is that results in Table 2.4 could be driven by a *field effect*: editors may tend to publish papers related to their field of research. As the field of research could constitute a factor determining the establishment of social ties, a significant effect of social ties on publication outcomes could not be entirely

[23] By comparing equation 2.6 and equation 2.7, it follows that $\beta_1 = (\gamma_1 - \gamma_2)/2$.

[24] For instance, in the first issue of the QJE of 2005, Alberto Alesina was replaced as editor by Robert J. Barro; however, given the long review process applied by this journal, it is unlikely that the papers in that particular issue were chosen and reviewed by the newly appointed editor. The variable $InCharge^{entry}$ should also account for temporal lags when matching editors and authors.

imputed to the existence of the social connection with the editor. To control for this confounding factor, I estimate whether the number of papers published in journal j that are in the same research field as the one of the editor increases when the editor is in charge. Column (1) of Table 2.6 provides estimates of this last set of regressions: the effect is negative but not significant.

The social effect found in the previous section seems not to be driven by a field effect. In columns (2) and (3) of the same Table I distinguish between *same field* articles that are authored by university colleagues of the editor, i.e. *Same faculty* connections, and those authored by researchers belonging to a different faculty. The coefficient in column (2) is positive and significant, suggesting that an editor is more likely to publish articles that are authored by scholars from his university and in the same research field. On the contrary the estimate in column (3) is negative and statistically significant: articles in the same field as the editor's and authored by scholars from another university decrease in terms in which the editor is in charge. Competition among scholars in the same research field across different institutions may be a potential explanation for these results. In Panel B of the same Table, the entry and exit effects of the editorship on the publication outcomes of connected scholars are only significant for faculty colleagues in the same research field, i.e. Column (2).

2.5.3 Effect of Connections on Articles' Citations

As highlighted in Section 2.2, there are two potential explanations for the preferential treatment of editors towards connected scholars. On the one hand, professional links foster cooperation between the editor and the author giving rise to technological complementarities, which ultimately improve the quality of the paper. On the other hand, editorial favouritism may arise because of tastes for connected authors independently of the quality of their work, possibly leading to a dilution in the quality of the publications.

The net effect of connections on the quality of papers is then ambiguous, the analysis of the ex-post citations a paper receives can shed light on the mechanisms driving the results previously found. Citations are a well established measure for a paper's quality although admittedly not perfect. One may argue that the number of citations a paper receives do not perfectly reflect its quality. For instance, innovative or controversial papers may need longer time than usual in order to get accepted by the scientific community and thus be cited. The data used in this work only refer to the total number of a paper's citations up to December 2012, thus papers published in the last year of the observation period have a 6-year time window in which they could be quoted. Another argument against the use of citations as a proxy for quality of a paper is that economists may strategically quote authors connected to a particular editor of a journal in order to increase the chances of getting a paper published in that journal. Despite these valid arguments, the number of citations still remains the most objective measure of the quality of papers; however, results in this section should be taken with caution.

Table 2.7 presents estimation results of equation (2.6) where the dependent variable is the average number of citations of connected papers published in journal j at time t.[25] The econometric specification is the same used in the previous sections and it includes editor and time-journal matches fixed effects. Standard errors are again clustered at the level of the interaction between the journal and the term.

Results show that the only significant coefficient is the one for the *same faculty* connected papers: when the editor is in charge, connected papers receive on average 8.6 more citations than in semesters in which he is not in charge. This result is consistent with editorial favouritism being driven by complementarities between authors' and editors' inputs. It seems that the cooperation and the communication between an editor and an author who work in the same institution and then interact on a daily basis, increase the quality of connected articles.

Interestingly there is no significant increase in citations for papers authored by former PhD students: the estimated coefficient is positive but statistically not significant. Editors' tastes for papers authored by former PhD students may offset the potential increase in quality generated by technological complementarities.

In Panel B I estimate equation (5), thus decomposing the overall effect of the editorship on paper's quality between entry and exit effect. The positive effect found in column (3) of Panel A seems to be driven by the fact that whenever the editor is no longer in charge, articles of his connections receive less citations.

Overall social connections do not negatively affect the quality of publications. On the contrary, working in the same institution as the editor increases both scholars' chances of publishing a paper in one of the top four economics journals and it also improves the quality of the paper.

[25] I decided to use the average instead of the total number of citations, as the latter may also reflect the increase in the number of connected papers published.

2.6 Conclusions

This work provides evidence on the role played by social ties between authors and editors in the publication process in economics. By employing a unique dataset on all the articles published in the leading American general interest journals in economics over the period 2000-2006, I find that, when a scholar becomes editor of a journal, his connections improve their publication outcomes, such as the number of articles, article's length and position within the journal issue.

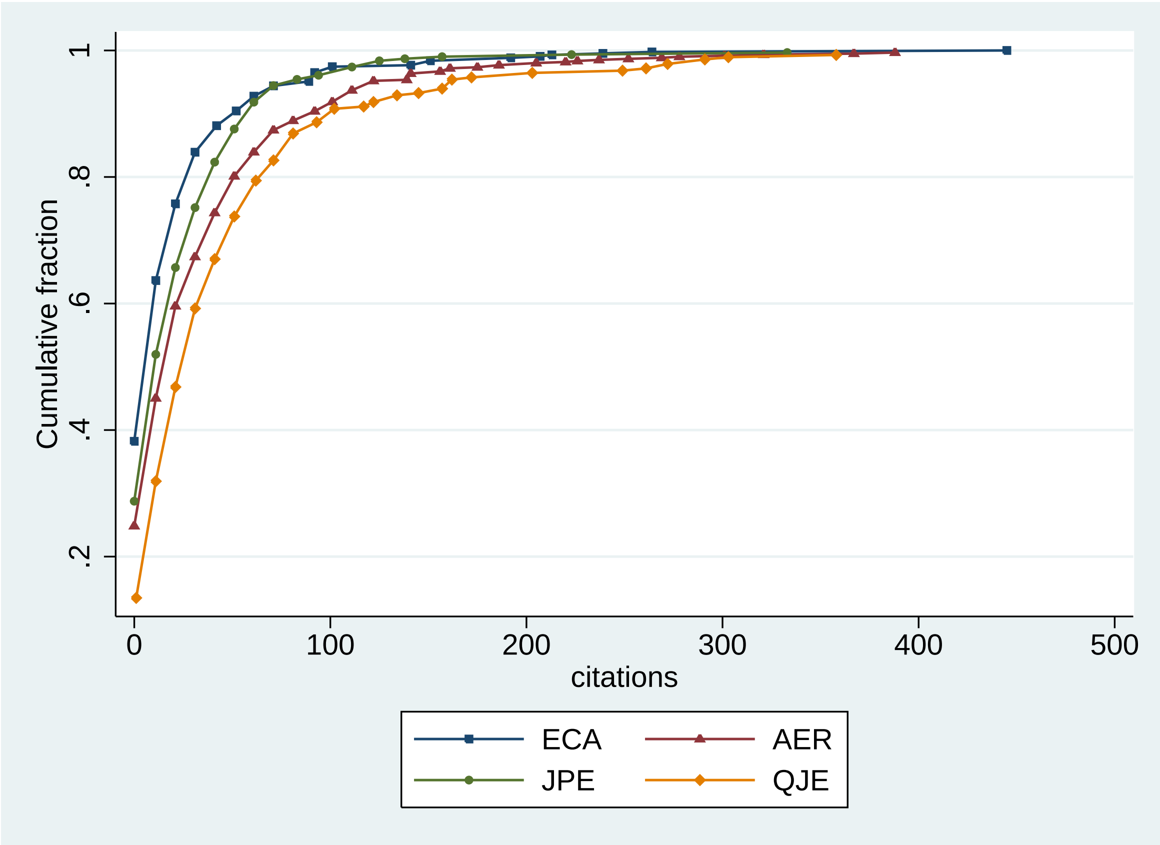
The social effect is particularly relevant for former PhD students and university colleagues of the editor; for instance, during an editor's appointment, the number of published articles written by former graduate students increase by 0.22 per semester. The coefficient for university colleagues is even higher: during an editor's mandate, working in the same institution as the editor increases the number of published articles by about a paper per year. The existence of a social tie with the editor influences other outcome variables of the publication process: for university colleagues, both the length of the article and the probability of having a paper published as lead article significantly increase. Interestingly, past co-authors of the editor do not benefit from the editor's appointment in terms of number of published papers; however, they improve their chances to have a paper published as lead article in the journal.

I developed a simple theoretical framework to illustrate how social ties affect editorial choices. Because of tastes and technological complementarities between connected scholars, editors always prefer to publish papers authored by researchers they are connected to. However, the sign of the differential quality of connected and non-connected papers is ambiguous.

In order to test for the implications of the model, I analyse the effect of social ties on the number of citations that papers receive. Articles by faculty colleagues of an editor receive on average 8.6 more citations when that editor is in charge, suggesting that complementarities between editors' and authors' effort in the publication process drive this result. There is no beneficial effect on citations from publishing papers authored by PhD students and past co-authors. The preferential treatment for these two types of connections does not lead to any improvement in the quality of published articles. In all cases analysed I never recover a negative effect of connections on the quality of the papers.

Tables and Figures

Figure 2.1: Cumulative distributions for citations to articles over the period 2000-2006, by journal



Notes: Data on citations were retrieved from *IDEAS-RePEc* in December 2012. They also include citations in working papers and self citations.

Figure 2.2: Authors' institution of appointment at the time of publication, by journal

Notes: The figure plots the academic institutions that account for the first 50% of published articles over the period 2000-2006.

FIGURE 2.3: Authors' institution of PhD award

Notes: The figure plots the PhD institutions of authors that account for the first 50% of published articles over the period 2000-2006.

TABLE 2.1: Articles' characteristics

	All journals	AER	ECA	JPE	QJE
Number of articles	1621	604	429	306	282
Number of pages	25.91	17.88	27.23	29.87	36.78
	[12.88]	[6.46]	[13.08]	[9.62]	[9.25]
Citations per article	37.93	42.01	25.79	31.93	54.16
	[57.32]	[57.68]	[39.41]	[54.20]	[76.32]
Number of authors	1.95	1.94	1.92	1.94	2.03
	[0.78]	[0.77]	[0.76]	[0.76]	[0.84]
Top 5 JEL codes:					
	C (17.97%)	E (15.84%)	C (53.35%)	D (19.56%)	J (12.73%)
	D (17.03%)	D (13.67%)	D (21.94%)	J (15.77%)	H (12.36%)
	J (11.31%)	J (13.02%)	G (6.7%)	E (11.67%)	D (12%)
	E (10.63%)	F (12.8%)	J (5.31%)	H (10.09%)	E (10.55%)
	H (8.48%)	H (10.63%)	E (4.39%)	G (9.46%)	I (10.55%)
Single author papers:					
Fraction	29.30	29.30	30.77	29.74	26.60
Citations per article	30.24	36.23	24.42	24.62	33.20
	[41.05]	[44.15]	[46.64]	[31.86]	[29.71]
Connected Papers:					
Same PhD	9.38	9.44	7.46	5.88	15.96
PhD Advisor	15.30	12.42	14.92	15.36	21.99
Same Faculty	17.21	14.57	18.18	16.01	22.70
Faculty Ever	27.88	24.34	33.10	29.08	26.24
Co-authors	10.55	6.62	12.82	8.50	17.73
Overall	43.55	37.75	47.79	41.83	51.42
HHI index	0.017	0.013	0.016	0.024	0.047

Notes: The table reports articles characteristics by journal for the period 2000-2006. Appendix A provides information on the *JEL* classification system. Connections are defined in Section 2.3.1. The Herfindahl index for journal j is defined as $HHI_j = \sum_i s_{ij}^2$, where s_{ij} is the fraction of all articles in journal j written by authors affiliated with institution i.

Table 2.2: Citations and articles' characteristics

	(1)	(2)	(3)	(4)
N Authors:				
2	6.5976***	6.5976***	5.7673**	6.7139**
	(2.4082)	(2.4082)	(2.4444)	(2.9814)
3	18.6473***	18.6473***	17.6378***	16.5458***
	(5.1357)	(5.1357)	(5.1031)	(5.6557)
4	32.1088***	32.1088***	31.5022***	30.7331**
	(12.0708)	(12.0708)	(11.9630)	(12.3535)
5	21.6406	21.6406	23.1440	24.2018
	(15.8451)	(15.8451)	(16.4137)	(17.9871)
Econometrics		-26.3398**	-29.5579**	-36.2690***
		(11.9695)	(12.3122)	(12.6731)
N Pages			0.9308***	0.9539***
			(0.2023)	(0.2216)
$NPages^2$			-0.0051***	-0.0056***
			(0.0011)	(0.0012)
Constant	64.3000***	64.3000***	55.2647***	36.4863***
	(12.0474)	(12.0474)	(12.7463)	(13.1994)
Controls:				
Journal	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
JEL Codes	Yes	Yes	Yes	Yes
Journal Issue FE	No	No	No	Yes
Observations	1,621	1,621	1,621	1,621

Notes: * p<0.10, ** p<0.05, *** p<0.01. Each observation is the article. Standard errors in brackets are clustered by journal issue. Econometrics is a dummy variable indicating whether the article's JEL classification is C.

Table 2.3: Authors' and editors' characteristics

	Authors	Editors
Number	1,828	42
Share of Males	89.55	92.31
Share of early career	23.49	1.92
Share of Professors	52.02	98.15
Top Nationalities:		
U.S.	40.1	67.31
Italy	5.5	1.92
Canada	5.13	5.77
Germany	5.01	3.85
UK	4.53	5.77
France	4.47	7.69
Top Institutions:		
Harvard	6.18	11.11
Chicago	5.74	27.78
MIT	4.79	5.56
Stanford	3.88	3.7
Princeton	3.61	5.56
Top PhD Institutions:		
MIT	11.29	32.69
Harvard	10.02	7.69
Chicago	6.1	5.77
Stanford	6.1	5.77
Princeton	4.89	5.77
Berkeley	4.17	1.92
Share of connected authors:		
Same PhD	20.65	.
Same Country	67.21	.
PhD Advisor	40.46	.
Same Faculty	48.31	
Faculty Ever	56.52	.
Co-authors	28.73	.

Notes: The table reports scholars characteristics for the period 2000-2006. Connections are defined in Section 2.3.1.

TABLE 2.4: Social ties and publication outcomes

	All Connections (1)	PhD advisor (2)	Same Faculty (3)	Faculty Ever (4)	Co-author (5)	Same PhD (6)
Panel A: Number of articles						
In Charge	0.3070**	0.2165**	0.5898***	0.1188	0.0900	0.0138
	(0.1286)	(0.0825)	(0.1566)	(0.1160)	(0.0671)	(0.0572)
Panel B: Number of pages						
In Charge	12.8910***	6.3933**	18.7392***	7.6161**	4.3092*	0.7261
	(4.4512)	(2.9531)	(4.8558)	(3.2437)	(2.1869)	(1.6795)
Panel C: Number of lead articles						
In Charge	0.0500	-0.0012	0.1003**	0.0464	0.0558***	-0.0090
	(0.0565)	(0.0290)	(0.0415)	(0.0458)	(0.0201)	(0.0231)
Panel D: Number of authors						
In Charge	0.4899***	0.2277**	0.6626***	0.1488	0.0812	0.0367
	(0.1641)	(0.0890)	(0.1698)	(0.1336)	(0.0705)	(0.0628)
Controls:						
Journal	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
Editor	Yes	Yes	Yes	Yes	Yes	Yes
Journal*Time	Yes	Yes	Yes	Yes	Yes	Yes
Editor*Journal	Yes	Yes	Yes	Yes	Yes	Yes
Observations	602	602	602	602	602	602

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Each observation is an editor*journal*time (ijt). In Panel A the dependent variable is the number of articles connected to editor i published in journal j in term t. The dependent variable in Panel B is the number of pages of connected articles. The probability of having a connected lead article in journal j in term t is analysed in Panel C. Finally in Panel D, the dependent variable is given by the number of connected authors that publish in journal j at time t. Standard errors in brackets are clustered at the level of the interaction between journal j and time t.

TABLE 2.5: Social ties and publication outcomes: persistent effects

	All Connections (1)	PhD advisor (2)	Same Faculty (3)	Faculty Ever (4)	Co-author (5)	Same PhD (6)
Panel A: Number of articles						
$InCharge^{entry}$	0.4189**	0.3662*	0.8920***	0.2111	-0.0433	-0.0128
	(0.2039)	(0.1854)	(0.2972)	(0.1860)	(0.1333)	(0.0901)
$InCharge^{exit}$	-0.1867	-0.0556	-0.2650**	-0.0195	-0.2333**	-0.0423
	(0.2345)	(0.1525)	(0.1240)	(0.1935)	(0.1116)	(0.1164)
Panel B: Number of pages						
$InCharge^{entry}$	15.9144**	7.5935	30.5251***	10.4897*	3.3994	0.5214
	(7.4282)	(5.9785)	(9.5620)	(5.9834)	(4.0331)	(2.7893)
$InCharge^{exit}$	-9.6407	-5.1029	-6.0689*	-4.5268	-5.2873	-0.9461
	(7.1948)	(4.6064)	(3.3408)	(5.1351)	(3.3782)	(3.1545)
Panel C: Number of lead articles						
$InCharge^{entry}$	-0.0985	-0.0133	0.1611*	-0.0587	0.0138	0.0162
	(0.0905)	(0.0518)	(0.0861)	(0.0791)	(0.0425)	(0.0344)
$InCharge^{exit}$	-0.2097**	-0.0117	-0.0348	-0.1593**	-0.1009*	0.0360
	(0.0988)	(0.0564)	(0.0343)	(0.0759)	(0.0511)	(0.0353)
Panel D: Number of authors						
$InCharge^{entry}$	0.6556**	0.3812*	0.9875***	0.2549	-0.0534	0.0123
	(0.2912)	(0.1924)	(0.3358)	(0.2550)	(0.1472)	(0.0952)
$InCharge^{exit}$	-0.3118	-0.0628	-0.3134**	-0.0347	-0.2258*	-0.0631
	(0.2949)	(0.1627)	(0.1508)	(0.2324)	(0.1214)	(0.1309)
Controls:						
Journal	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
Editor	Yes	Yes	Yes	Yes	Yes	Yes
Journal*Time	Yes	Yes	Yes	Yes	Yes	Yes
Editor*Journal	Yes	Yes	Yes	Yes	Yes	Yes
Observations	602	602	602	602	602	602

Notes: * p<0.10, ** p<0.05, *** p<0.01. Each observation is the editor*journal*time (ijt). $InCharge_{ijt}^{entry}$ is a dummy that is equal to one at any time following editor j's appointment. $InCharge_{ijt}^{exit}$ is another dummy that is one at any time t after editor j finishes his mandate.The dependent variables are described in Table 2.4. Standard errors in brackets are clustered at the level of the interaction between journal j and time t.

Table 2.6: Robustness check: same field

	Same Field	Same Field & Faculty	Same Field & diff. Faculty
	(1)	(2)	(3)
Panel A: Number of articles			
In Charge	-0.2837	0.1960***	-0.4797**
	(0.1882)	(0.0596)	(0.1943)
Panel B: Number of articles and persistent effects			
$InCharge^{entry}$	0.0078	0.2615**	-0.2537
	(0.4273)	(0.1054)	(0.3974)
$InCharge^{exit}$	0.5972	-0.1255***	0.7226
	(0.5010)	(0.0467)	(0.4922)
Controls:			
Journal	Yes	Yes	Yes
Time	Yes	Yes	Yes
Editor	Yes	Yes	Yes
Journal*Time	Yes	Yes	Yes
Editor*Journal	Yes	Yes	Yes
Observations	602	602	602

Notes: * p<0.10, ** p<0.05, *** p<0.01. Each observation is the editor*journal*time (ijt). See notes to Table 2.4 and Table 2.5.

TABLE 2.7: Social ties and citations

	All Connections	PhD advisor	Same Faculty	Faculty Ever	Co-author	Same PhD
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average citations						
In Charge	-2.1400	1.0119	8.6037***	-3.3903	-5.0794	-0.2626
	(3.4730)	(2.4270)	(2.7616)	(4.2710)	(5.0429)	(2.9195)
Panel B: Average citations and persistent effects						
$InCharge^{entry}$	0.7840	8.5495	10.9486**	-1.1785	-7.7812	-2.4639
	(5.0262)	(7.0056)	(5.2814)	(6.6088)	(7.9624)	(5.9383)
$InCharge^{exit}$	5.2835	7.0914	-6.0828**	5.7681	2.1748	-2.1039
	(4.5299)	(6.7794)	(2.8778)	(4.4411)	(5.5773)	(3.6320)
Controls:						
Journal	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
Editor	Yes	Yes	Yes	Yes	Yes	Yes
Journal*Time	Yes	Yes	Yes	Yes	Yes	Yes
Editor*Journal	Yes	Yes	Yes	Yes	Yes	Yes
Observations	602	602	602	602	602	602

Notes: * p<0.10, ** p<0.05, *** p<0.01. Each observation is the editor*journal*time (ijt). The dependent variable in both Panels is the average number of citations that articles published in journal j at time t receive. Standard errors in brackets are clustered at the level of the interaction between journal j and time t.

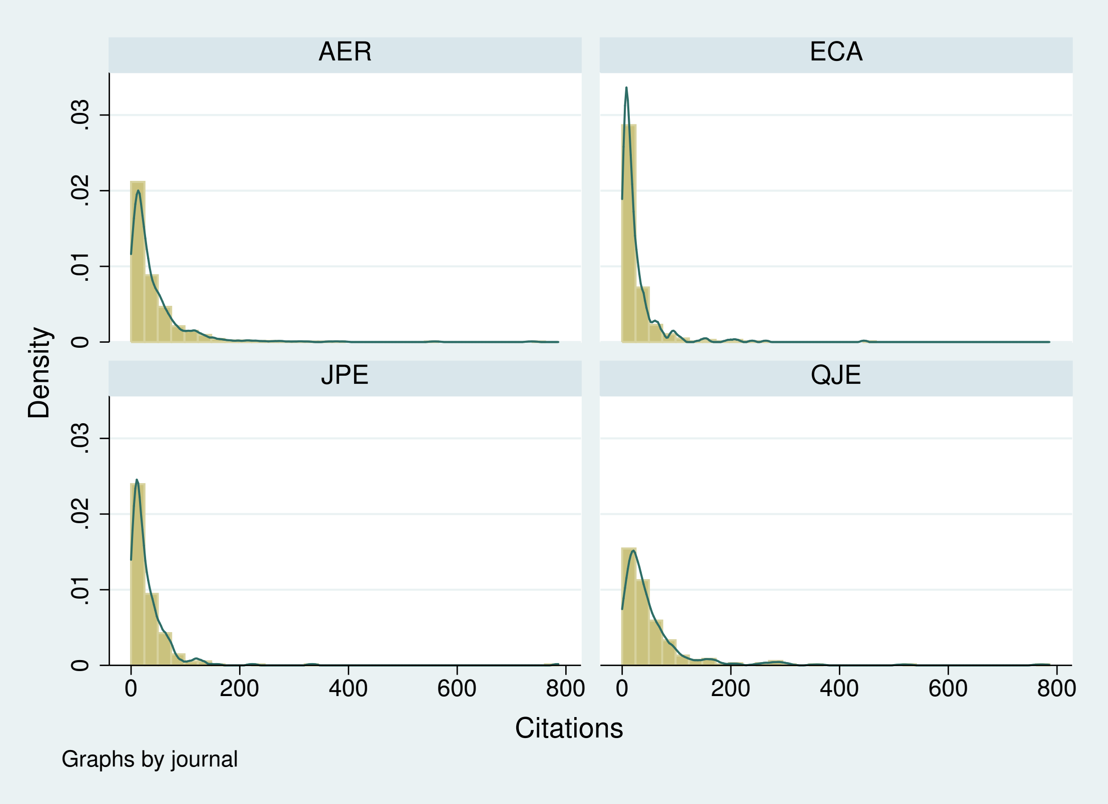
Appendix A: *JEL* Classification

Articles in this dataset are classified according to the JEL classification codes, a system that has been implemented by the Journal of Economic Literature (*JEL*). There are 19 JEL categories, these are:

JEL: A - General Economics and Teaching
JEL: B - History of Economic Thought, Methodology, and Heterodox Approaches
JEL: C - Mathematical and Quantitative Methods
JEL: D - Microeconomics
JEL: E - Macroeconomics and Monetary Economics
JEL: F - International Economics
JEL: G - Financial Economics
JEL: H - Public Economics
JEL: I - Health, Education, and Welfare
JEL: J - Labor and Demographic Economics
JEL: K - Law and Economics
JEL: L - Industrial Organization
JEL: M - Business Administration and Business Economics; Marketing; Accounting
JEL: N - Economic History
JEL: O - Economic Development, Technological Change, and Growth
JEL: P - Economic Systems
JEL: Q - Agricultural and Natural Resource Economics; Environmental and Ecological Economics
JEL: R - Urban, Rural, and Regional Economics
JEL: Y - Miscellaneous Categories
JEL: Z - Other Special Topics

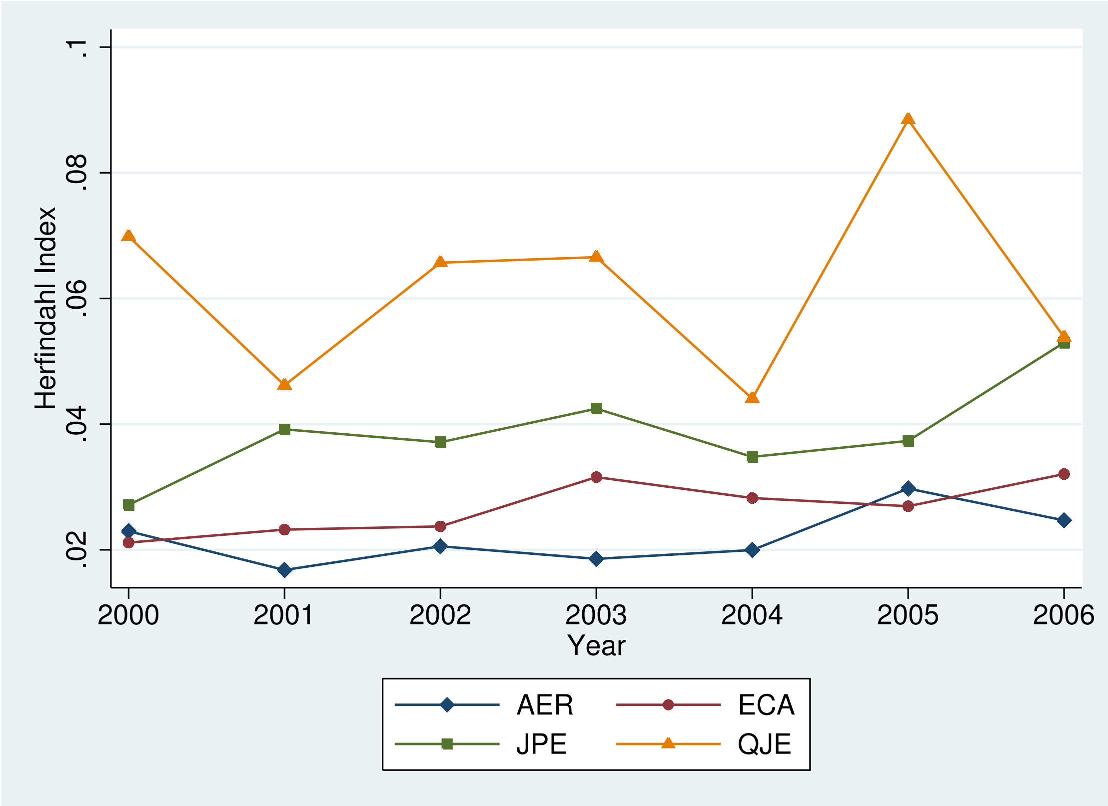
Appendix B: Supplementary Tables and Figures

Figure B.1: Articles citations up to december 2012 by journal



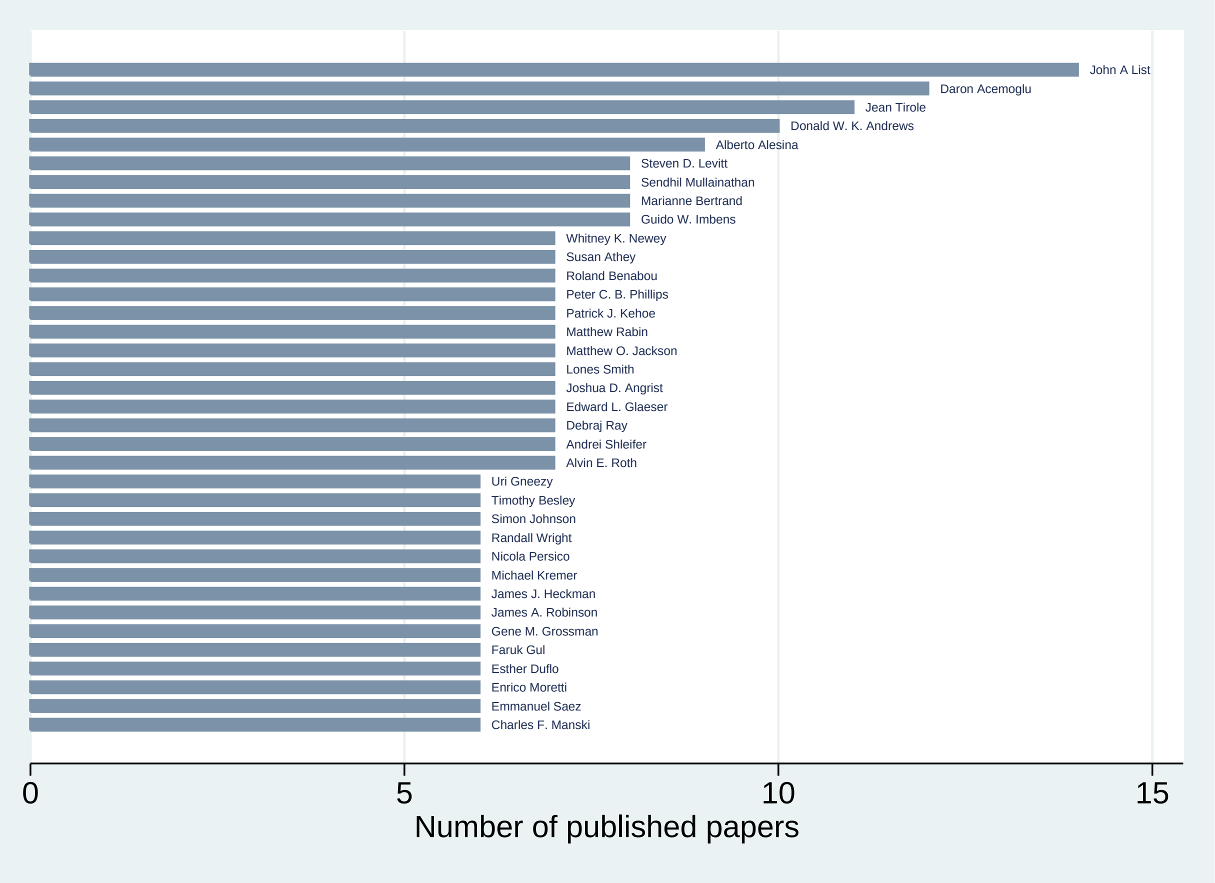
Notes: Data on citations were retrieved from *IDEAS-RePEc* in December 2012. They include citations in working papers and self citations. The green line plots the Kernel density function.

Figure B.2: HHI Index by year and journal



Notes: The Herfindahl index for journal j at time t is defined as $HHI_{jt} = \sum_i s_{ijt}^2$, where s_{ijt} is the fraction of all articles in journal j written by authors affiliated with institution i in year t.

Figure B.3: Most published authors, over 2000-2006



Notes: The figure plots the number of articles published by authors in the sample in any journals analysed over the period 2000-2006. The figure only reports authors that published more than four articles in the observation period.

Figure B.4: Most published authors by journal, over 2000-2006

Notes: The figure plots the number of articles published by authors in the sample in any journals analysed over the period 2000-2006. The figure only reports authors that published more than four articles overall.

Chapter 3

Employment Outcomes and Female Managers: Evidence from Takeovers

3.1 Introduction

This paper employs a matched employer-employee longitudinal dataset covering the universe of private sector employees for twenty-seven years in Italy to study the impact of female managers on female workers' employment outcomes and on firms' economic performance.

Several studies have shown that executives' characteristics strongly influence management practices and thus firms' behaviour, partially explaining differences in the productivity and the performance across firms operating in the same country and in the same sector of activity (Bertrand and Schoar 2003; Bloom and Van Reenen 2007). Kaplan, Klebanov and Sorensen (2012), analysing a dataset on individual CEOs of private companies in the US, find that a firm's economic performance is positively and significantly correlated to specific CEOs' characteristics such as resoluteness, aggressiveness and overconfidence; on the contrary, executives' traits, such as empathy, openness to criticism and communication have a detrimental effect on the firm's economic success.

Psychological attributes have also been investigated in the labour economics literature as they constitute a potential explanation for females' *occupational segregation* and for the *gender wage gap* (Bertrand 2010). Male and female workers exhibit different attitudes towards risk: women are more risk adverse and less keen to competition, while men tend to behave more aggressively in the work environment. For instance, Gneezy, Niederle and Rustichini (2003) present experimental evidence that, whenever the competitiveness of the environment increases, men's performance significantly improves; but no effect is found in women. Therefore, occupations that provide competitive compensation schemes, such as executive positions, may be more attractive for men than for women, potentially explaining why female workers are underrepresented in top positions in the firm and at the top of the wage distribution, the so called *glass ceiling* phenomenon (Albrecht, Bjorklund and Vroman 2003). In the U.S., women only account for less than 3% of high-level managers, earning about 45% less than male executives (Bertrand and Hallock 2001); such differences can also be found in Europe, where the share of female directors was about 8 percent in 2004 (Adams and Ferreira 2009).

Several papers in both labour economics and political economy have investigated the effect of female leaders on females' outcomes in the labour market and in politics respectively. There is an established literature in political economy providing evidence of gender differences in how policy making is done: female politicians invest more in public goods that address women's concerns; they are also better than men in delivering policy outcomes, such as reducing the size of the budget deficit (Gagliarducci and Paserman 2012). Moreover, women in politics tend to reduce administrative irregularities and corruption (Brollo and Troiano 2012). Chattopadhyay and Duflo (2004) study the impact of women's leadership on policy decisions by exploiting an experiment in India that reserved one-third of Village Council head positions to women. This reform led Village Councils to adopt policy decisions in ways that better reflected women's preferences. Legislative initiatives promoting women's access to public office, such as *gender quotas*, are believed to help reducing gender stereotypes in politics: if voters realize that women can be effective leaders, the presence of women in politics will eventually increase (Beaman, Chattopadhyay, Duflo, Pande and Topalova 2005).

The literature on the impact of female executives on workers' and firms' performance is relatively scarce. A first set of studies tries to answer the question of whether women are better managers than men (Smith, Smith and Verner 2013; Adams and Ferreira, 2009), arguing that gender differences in risk aversion and competitiveness may result in different management practices and thus different firms' economic outcomes. Empirical results are ambiguous: an increase in the female presence on corporate boards positively influences male executives' attendance rates; however, the overall effect on firms' economic performance is negative. Gender (and race) diversity among directors also affects the composition and characteristics of the workforce in the firm. Giuliano, Levine and Leonard (2009) provide empirical evidence of the effect of demographic similarities between managers and subordinates on subordinates' employment outcomes: having a same-race manager increases a worker's chances of promotion and it reduces quits and dismissals. Along these lines, Weber and Zulehner (2010, 2013) address the question of whether workplace gender composition has an impact on firms' success: they find that firms employing a low fraction of women relative to the industry's average have a higher probability of closure.

A different set of papers studies whether female-led firms protect female employees by paying them higher wages compared to firms in which managers are men. Bell (2005) analyses the gender gap in top executive jobs, finding that female executives working in firms managed by female CEOs earn about 20% more in total earnings than women working in other firms. Flabbi, Macis and Schivardi (2012), employing a similar version of the dataset used in this work, show that the effect of female leadership on different measures of firm economic performance is positive;[1] they also find that the interaction between female managers and female workers leads to an increase in wages of women at the top of the wage distribution, while they observe a decrease in wages of female workers at the bottom of the same distribution. Among different mechanisms behind these findings, a plausible one is represented by gender based discrimination: female executives correct biases affecting women in the firm, ultimately preventing job mismatches and potentially increasing firms' economic performance (Cardoso and Winter-Ebmer, 2010).

The main empirical challenge of this type of analysis is given by the non-random assignment of workers to firms. Workers may self select into firms according to unobservable characteristics, which are correlated with their employment outcomes. Most of the papers mentioned above provide compelling evidence of a positive correlation between female leadership and the economic success of both female workers and firms; however, they account for non-random sorting by conditioning on a large set of individuals' and firms' characteristics (Flabbi, Macis and Schivardi 2012; Weber and Zulehner 2013). Clearly the validity of this identification strategy relies on the hypothesis that workers' mobility is uncorrelated with latent changes in the outcome variable.

[1]Flabbi, Macis and Schivardi (2012) use a matched employer-employee dataset from Italy, which includes information on the entire labour force of a large sample of firms in the manufacturing sector over the period 1982-1997. The dataset also provides balance sheet information for every firm.

In this paper I employ a different estimation strategy, I use a differences-in-differences approach that exploits variations in firms' workforce composition generated by *firms' takeovers.* I use an Italian law regulating takeovers, which requires acquiring firms to retain workers employed in the acquired firm at the same wage and employment conditions as in the pre-acquisition period. I then compare outcomes of firms that acquired companies with a high share of female managers (*treatment* group) to companies with a low share of female managers (*control* group), before and after the acquisition took place. This strategy allows me to estimate how female workers' employment outcomes vary depending on the share of acquired female managers. The longitudinal nature of the dataset allows me to condition on a large set of controls for both acquiring and acquired firms. Moreover, when examining individual workers' employment outcomes, the inclusion of individual fixed effects washes out any effect due to unobserved individual characteristics.

For this empirical exercise I employ matched employer-employee micro dataset from the administrative records of the Italian Social Security Administration (INPS) for the Italian region of Veneto (also used in Card, Devicienti and Maida 2011; Colussi 2013), which covers the universe of private non-agricultural dependent employment relationships between January 1975 and December 2001. The data provide detailed information on each employment relationship, such as annual gross earnings, weeks worked and occupational status. The information on firms includes the sector of activity, the location and the date of closure.

The first set of results shows that an increase in the share of acquired firms' female managers significantly increases the proportion of female managers in the acquiring firm in the 24 months after the takeover, without affecting the share of women employed in other occupations, such as blue and white collars. These results are robust to the inclusion of both acquired and acquiring firms' baseline characteristics interacted with a post-takeover dummy, which should control for potential correlations between latent trends in the outcome variables and the share of female workers in the acquired firm.

Results on firms' outcome variables, such as survival probabilities and the number of employees, are small in magnitude and not statistically significant: female managers seem not to affect these two measures of firm economic performance. However, I find a negative and statistically significant effect of a change in the share of female managers on acquiring firms' wage dispersion, measured by the variance of log wages.

The analysis of individual workers' employment outcomes shows that the share of acquired female managers has a negative effect on the job retention probability of acquiring firm's incumbent workers; however, estimated coefficients are never statistically significant. Further, I do not find any effect on either female and male employees' earnings.

Despite the large literature documenting a positive effect of female leadership on women's employment outcomes, this paper does not find a statistically significant effect on the different outcome variables analysed. However there is an interesting negative effect on wage inequality within the acquiring firm, which may matter for both equity and efficiency reasons.

The remainder of the paper is organised as follows. Section 3.2 presents the data and the institutional background. Section 3.3 describes the empirical strategy. I present and discuss the empirical results in Section 3.4. Section 3.5 concludes.

3.2 Data and Takeovers

The data used in this work are matched employer-employee micro data from the administrative records of the Italian Social Security Administration (INPS) for the Italian region of Veneto. The dataset covers the universe of private non-agricultural employment relationships between January 1975 and December 2001; it provides information about start and end dates of each employment relationship, the total compensation paid in each year, the number of working weeks, the type of contract (part-time vs. full time), and the job occupation. The information on workers' characteristics includes the age, the gender, and the city of birth. The data also report firms' sector of activity (at the 3 digit level) and the municipality in which firms are located.[2]

The primary unit of observation in the data is a firm-worker match for each calendar year. In other terms, for each employment relationship, there are as many observations in the data as the number of calendar years over which this relationship spans. In each calendar year, there can be multiple observations for each

[2]Veneto is one of twenty Italian regions located in the north east of Italy; its total population in 2012 was of about five million, ranking fifth in Italy. The most populated city is Venice, i.e the capital of the region, registering about 270,000 inhabitants. While being one of the poorest agricultural region and one of the largest sources of immigration to the U.S. and Americas, Veneto experienced an impressive industrialisation, being now the third richest region in terms GDP in Italy (Tattara and Anastasia 2003).

individual, as individuals can hold more than one job, whether simultaneously or sequentially, during the same year. Both workers and firms in the data are individually identifiable and can be followed over time. The dataset follows every worker from the moment they first started working in Veneto, even if they subsequently find a job in a firm located in an Italian municipality outside Veneto. The absorbing state hence includes non-employment, death, movements to other countries, self-employment, public sector employment and informal employment. The data also exclude self-employed individuals or those employed in family businesses for which registration at INPS archive is not required. The original dataset includes information on around 3.6 million workers for a total number of approximately 12.5 million employment relationships in more than 1.1 million firms (Colussi 2013).

3.2.1 Takeovers and Institutional Background

The dataset also provides information on firms' takeovers: a specific variable indicates the calendar date (month) in which a firm stopped operating as a consequence of an acquisition by another firm. Acquisitions reported in the dataset only consist of incorporations in which acquired firms disappear from the dataset as an independent economic entity and become part of the acquiring firm.

As mentioned in the introduction, I exploit variations in the workforce composition of firms generated by takeovers. In Italy there is a specific law that regulates this type of takeovers, i.e. *Art. 47 of law 29/12/1990 n. 428*. This law states that, whenever a business is transferred from one owner to another one because of a takeover, acquired workers' terms and conditions of employment are automatically transferred to the acquiring firm. In case of dismissal by the new employer, individuals formerly employed in the acquired firm have the priority in the new hires of the firm within a year from the takeover. The firing restrictions applied to the new employer remain unchanged and are regulated by the the "Chart of Workers' Rights" (*Law No. 300: Statuto del Lavoratori*) of 1970, which severely restricted firing decisions of firms with more than 15 employees. This law states that in case of unfair dismissals, firms are forced to take back dismissed workers and to pay them their full wage before the lay-off. Moreover, firms are fined up to 200% of fired workers' original wage for the delayed payment of contributions.

Table 3.1 reports characteristics of both acquiring and acquired firms in the month before the takeover. Overall we observe 3,928 acquired firms and 3,291 acquiring firms, implying that some "buyers" acquired more than one firm in the observation period; the maximum number of takeovers performed by a single acquiring firm is eleven. By comparing acquiring and acquired firms, it can be noticed that acquiring firms are on average bigger than acquired firms, i.e. 173 versus 44.13 employees. More than 173,000 workers were employed by acquired firms in the month before the acquisition; while about 570,000 employees were working in acquiring firms at the month prior to the takeover. The table also provides descriptive statistics on all the firms observed in the data.[3] Acquiring and acquired firms are larger than the average firm operating in Veneto, as the average firm size in Veneto is about seven. Moreover, the average (gross) weekly wage paid by all firms in Veneto is 137 euros lower than the one paid by acquiring firms (and 73 euros lower than the one in acquired firms).

Unsurprisingly, acquiring firms are older than acquired ones: the average age of acquiring firms at the time of the acquisition is about seven years; while acquired firms are taken over when they are about four years old.[4]

Looking at the distribution of workers across occupations, there are small differences between the two types of firms. Acquiring firms have on average more white collars and managers than the acquired ones, while the number of blue collar employees is larger for the acquired ones, i.e. 66% versus 62%. The share of firms in which there is at least one top manager (*dirigente*) is very low for both types of firm, i.e. 9.08% and 15.82% respectively.

The data do not provide information on the owner of the firm. Further, they only directly identify top managers in the firm. For this reason, I define as managers, workers that are in the 95th percentile of the wage distribution within the firm, i.e. the highest paid 5 percent. The share of female top paid workers is still quite low, being only 17% in the acquiring firms and 28% in the acquired firms. From now on I will refer to top paid workers as *managers*.

Finally, the distribution across industries of acquiring firms largely mimics that of acquired firms, with the majority operating in the clothing industry (12.2%), shoes manufacturing (5.5%) and machinery (5.2%); about 70% of the acquired firms operate in the same industry as the firm buyer. It is interesting to notice that most of the firms analysed are in the manufacturing sector. The dataset also provides a specific variable indicating the month in which a firm stops its activity and disappears from the data, i.e. *firm closures*. This variable distinguishes between

[3] These characteristics are measured at the median date over the period.

[4] This information is recovered through a variable indicating the exact year in which a firm was established.

real closures and other events affecting a firm's business other than closures, such as changes in the name and in the organisation, break ups, and mergers. Overall 4.9% of firms do not survive in the 24 months following the acquisition event.

In the empirical analysis I examine different outcome variables of firms and workers both before (12 months) and after (24 months) the acquisition. To control for right and left censoring I restrict the analysis to takeovers that took place between January 1976 and December 1999, excluding the first year and the last two years of the dataset and thus allowing each firm to have at a 36-month time window.

Figure 3.1 plots the monthly acquisition rate for the period 1976-1999, i.e. the number of firms acquired every month over the total number of active firms. The positive trend observed in this figure reflects the growth in the number of takeovers that started in Italy in 1993 and reached its peak in 2001 (Napolitano 2003).

3.3 Identification Strategy

The aim of this section is to estimate the effect of an increase in the share of female managers on female workers' employment outcomes and on firms' economic performance.

Simple OLS estimates of this relationship are unlikely to lead to consistent estimates of the parameter of interest. Clearly, issues as *omitted variables* and *reverse causality* are pervasive in this relationship; for instance, more or less female intensive firms might have different outcomes for reasons other than the share of female managers in those firms. To solve these identification issues, I exploit the variation in the share of female managers generated by takeovers, in line with the legislative setting presented in Section 3.2.

In particular, I estimate how acquiring firms' outcomes vary before and after the acquisition event takes place, depending on the share of female managers in the acquired firm. I only restrict the sample to firms that have acquired at least another company within the observation period analysed. By denoting t the calendar date and t_0 the month of the takeover, I estimate the following equation:

$$y_{jftt_0} = \gamma_0 + \gamma_1(SF_{ft_0} * d_t) + \gamma_2 d_t + \gamma_3 SF_{ft_0} + X'_{jt_0}\gamma_4 + X'_{ft_0}\gamma_5 + \lambda_t + e_{jftt_0} \quad (3.1)$$

The dependent variable y_{jftt_0} is the outcome of firm j that bought firm f, measured at any time t. SF_{ft_0} represents the share of female managers employed in the acquired firm one month before the acquisition, while $d_t = I[t > t_0]$ is an indicator for the time after the takeover. The coefficient of interest is γ_1, which measures the effect of acquired firms' share of female managers on the outcome variable considered. X_{ft_0} and X_{jt_0} are baseline characteristics of acquired and acquiring firms respectively, which are measured in the month prior the takeover; these characteristics include: industry, firm size, average wage, share of female employees and share of females employed in every occupation. Finally λ_t are time fixed effects. The specification can be augmented with takeover fixed effects, which account for unobserved characteristics of each acquisition.[5] This model can be seen as a sort of reduced form equation where the outcome variable is regressed on the instrument, i.e. the share of female managers in the acquired firm.[6]

Equation 3.1 is a differences-in-differences model that compares outcomes of firms that have acquired firms with different shares of female managers, before and after the takeover. The main identification assumption is that the share of acquired firm managers is not correlated with latent trends of the outcome variable analysed. The obvious econometric challenge is to test whether latent trends of the outcome variable for the control and the treatment would not have been different from each other in the absence of the acquisition event. In order to partially account for this, I include controls for baseline characteristics of the firm, both acquired and acquiring, interacted with the indicator variable d_t (Duflo 2003).

The dataset also provides information on individual workers' outcomes and characteristics. I can then estimate equation (3.1) at the individual worker level, this is:

$$y_{ijftt_0} = \gamma_0 + \gamma_1(SF_{ft_0} * d_t) + \gamma_2 d_t + \gamma_3 SF_{ft_0} + X'_{jt_0}\gamma_4 + X'_{ft_0}\gamma_5 + \lambda_t + \eta_i + e_{ijft} \quad (3.2)$$

where y_{ijftt_0} is the outcome variable, such as the wage of worker i employed in firm j, which acquired firm f. The inclusion of individual fixed effects, η_i, controls for unobservable individual characteristics, such as the ability of worker i.

[5] There might be different types of takeovers, for instance a takeover can be *hostile* or *friendly*. Depending on the type of takeover, the outcomes of workers and the performance of the acquiring firm may vary. As the information about the nature of acquisitions is not disclosed in the data, takeover fixed effects help control for this confounding factor. A takeover fixed effect is different from a firm fixed effect as a single firm can perform more than one takeover.

[6] I prefer not to present IV estimates as in principle the share of acquired female managers may affect outcomes other than the share of female managers. In all cases the reader can easily recover IV estimates.

3.4 Results

The first part of this section presents estimates of the effect of female managers on acquiring firms' workforce composition and economic outcomes. Every acquiring firm is observed for a total of 36 months (from 12 months before to 24 months after the acquisition event); the total number of observations is equal to 143,440. In every specification adopted, standard errors are clustered at the level of takeover.

Table 3.2 reports estimates of equation (3.1); the first dependent variable I examine is the share of female managers in the acquiring firm, i.e. Panel A. The coefficient of interest, γ_1, measures how the share of female managers in the acquiring firm after the takeover changes depending on the share of female managers employed in the acquired firm (effectively a first stage regression). The coefficient is positive and statistically significant suggesting that a ten percentage point increase in the share of acquired female managers increases the proportion of female managers in the acquiring firm after the takeover by 0.5 percentage points. The estimated coefficient remains positive and significant when takeover fixed effects are added to the regression (Panel A, column (2)).

In Panel B and C I look at acquiring firms' share of female blue and white collars. The coefficient on the treatment variable after the takeover is positive and statistically significant for both dependent variables, implying that the share of acquired female managers also affects the presence of female workers in other occupations.

In column (3) baseline characteristics of both acquired and acquiring firms, measured at the month prior the acquisition, are interacted with the indicator for the period following the takeover. These baseline characteristics are: firm size, average wage, average age, share of females, share of blue and white collars, share of female blue and white collars. Once these controls are included, coefficients for the share of female blue and white collars turn to be negative and no longer statistically significant, i.e. columns (3) to (6). On the contrary, the estimated coefficient for the share of female managers stays highly significant and positive, increasing from 0.05 to 0.07. This empirical strategy relies on a comparison in outcomes among firms that are identical at the baseline in terms of these observable characteristics.

The bottom panels of Table 3.2 report estimates of the effect of female managers on the overall composition of the acquiring firm's workforce. In Panel D, the share of managers (both female and male) increases as the share of acquired female managers rises. The estimated coefficients are always significant at the 5% level, implying that a ten percentage point increase in the share of acquired female managers raises acquiring firms' share of managers by 0.15 percentage points. Similarly, the effect on the fraction of white collars is positive and significant, being equal to about 0.03. Unsurprisingly, I find a negative effect on the proportion of blue collars: a ten percentage point increase in the share of acquired female managers decreases the share of blue collars by 0.3 percentage points. The presence of female managers has thus a significant impact on the distribution of workers across occupations. A possible explanation is that female executives are more likely to promote workers to higher occupations.

According to the existing literature, female executives are likely to correct previous job mis-allocations of female workers, potentially improving their performance at the firm. It is then worth investigating whether female managers ultimately affect firms' economic outcomes. The data do not provide balance sheet information hence, as measure of firm economic performance, I use the firm size and the survival probability within 24 months following the takeover. Overall only 4.9% of acquiring firms shut down in this time window.

Estimated coefficients on firms' survival probability are positive but very small in magnitude and never statistically significant. Further, I find that firms that survive in the 24 months subsequent the takeover, are more likely to experience a reduction in the number of their employees (Panel B). However, when I include controls for baseline firms' characteristics, estimated coefficients decrease in magnitude, turning statistically not significant.

Given the results found on the distribution of workers across occupations after the takeover, in the last Panel of Table 3.3 I estimate whether the increase in the share of female managers also has an effect on intra-firm wage dispersion. The dependent variable in Panel C is the variance of the log wages. Estimated coefficients are negative and statistically significant in every specification adopted: a ten percentage point increase in the share of acquired female managers reduces wage dispersion by about 0.3 percentage points. This last Panel shows that the effects on within-firm wage inequality are consistent with the effect of female managers on the distribution of workers across occupations.

3.4.1 Incumbent Workers' Outcomes and Acquired Female Managers

In this subsection I focus on the differential effect between male and female workers. Other papers on this topic (Flabbi, Macis and Schivardi 2012) claim that an increase in the number of women in top managerial positions positively affects promotions and wages of women within the firm. As employers are better at extracting information from workers of their same gender, an increase in the number of female executives reduces the mis-allocation of women to jobs, eventually improving their employment prospects.

Table 3.4 reports estimate of equation (3.2), where I focus on workers employed in the acquiring firm in the month before the takeover, i.e. *incumbent workers*. The total number of observation is equal to more than twenty million. In every specification I include baseline characteristics of both acquiring and acquired firms interacted with a dummy indicating the months after the takeover. Standard errors are again clustered at the takeover level.

Columns (1) and (2) of Table 3.4 examine the effect of the proportion of acquired female managers on incumbent workers' job retention probability after the takeover. In column (1) individual workers' characteristics and takeover fixed effects are included: as suggested by results on the firm size reported in Table 3.3, the estimated coefficient is negative and not statistically significant. In column (2) I allow the treatment variable to vary by workers' gender, then interacting it with a dummy indicating whether individual i is female. The estimated coefficient is very small (-.002), negative and again not statistically significant. When individual fixed effects are included in Panel B, estimated coefficients remain largely unchanged, being small in magnitude and not statistically significant.

In columns (3) and (4) of Table 3.4 the dependent variable is the probability of switching job, which is equal to one when the incumbent worker is employed by another firm. As expected, the coefficient is positive, but it is not statistically significant.[7] Results in column (4) does not show any differential effect between men and women on the probability of changing job.

The effect of female managers on incumbent workers' wage is positive, but small in magnitude and statistically not significant, i.e. column (5); it eventually turns negative when individual fixed effects are included. These results suggest that there is no effect of the increase in the number of female managers on incumbent employees' wages. Similarly, there is no effect of an increase in the share of acquired female managers after the takeover on wages of incumbent female workers, i.e. column (6). The coefficient on the interaction term is again negative and not

[7] Given that the estimated coefficients for the job retention probability and the for the probability of changing job are not statistically significant, the effect on the probability of non-employment is not significant as well; therefore it is not reported in the table.

significant; when individual fixed effects are added, the coefficient slightly increases in magnitude, still remaining statistically not significant.

The empirical findings in this section do not appear to be in line with what has been found in the previous literature. Among possible explanations for this divergence, one is that is that the time window analysed is too short for new female managers to make a difference. Another plausible explanation is that acquired managers eventually retained by the acquiring firm do not have the same decisional power as incumbent managers.

In sum, I do not find evidence of differential outcomes between male and female workers as the share of female managers in the acquiring firm increases.

3.5 Conclusions

In this paper I investigate whether a change in the share of female managers affects the employment outcomes of female workers within the firm. To this end, I employ a matched employer-employee longitudinal dataset covering the universe of private sector workers for twenty-seven years in Italy.

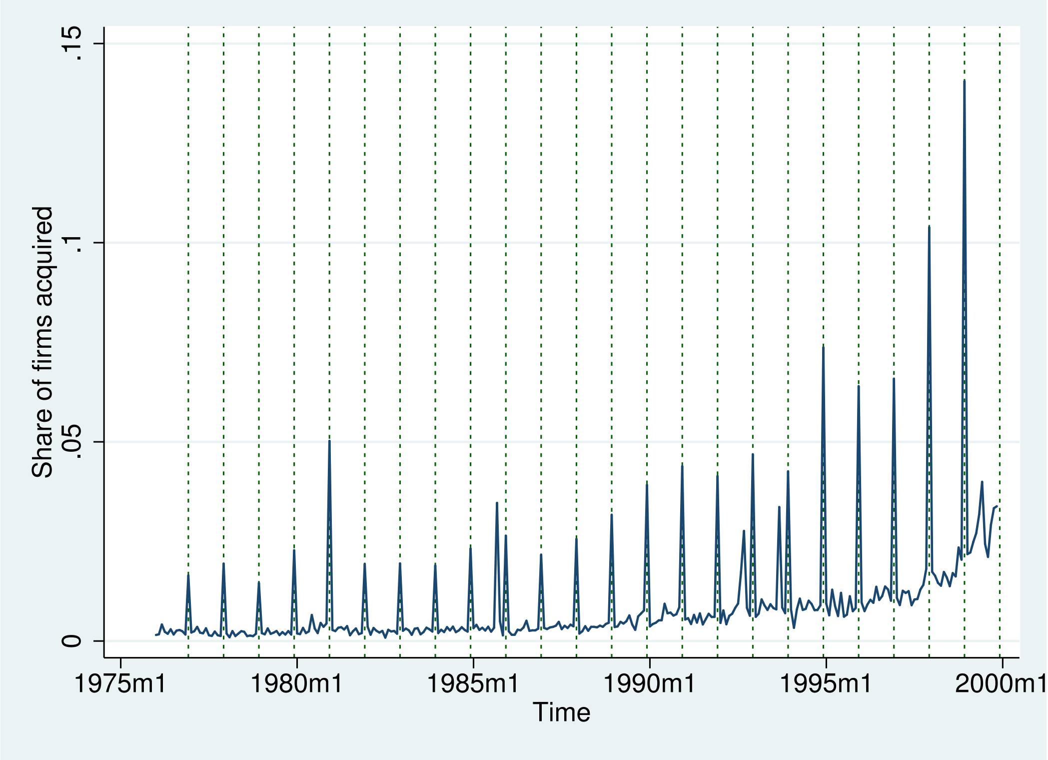
The main challenge to the empirical analysis is represented by the non-random sorting of workers to firms. As a solution, I use a differences-in-differences approach that exploits a variation in firms' workforce composition generated by takeovers. According to the Italian law regulating firms' acquisitions, whenever a business is transferred from one owner to another one because of a takeover, acquired workers' terms and conditions of employment are automatically transferred to the new employer.

I then compare economic outcomes of firms that acquired companies with a high share of female managers (*treatment* group) to those of firms that acquired few or no female managers (*control* group), before and after the acquisition took place. Estimated coefficients of the share of female managers on the firm's size and firm's survival probability are not statistically significant. However, there is a significant effect of the increase in share of female managers on the distribution of workers across occupations within the acquiring firm: the proportion of blue collars decreases, while the one of white collars and managers rises. This change in the occupational distribution ultimately reduces within-firm wage inequality.

The analysis of individual workers' employment outcomes, such as job retention probability and wages, does not provide significant results for either male or female workers.

Tables and Figures

Figure 3.1: Share of acquired firms over total active firms



Notes: author's calculations on INPS data; the share of acquired firms is computed as the number of acquired firms over the number of existing firms in every month of the observation period January 1976- December 1999. Dotted vertical lines indicate the month of December.

Table 3.1: Descriptive statistics

	Acquired Firms	**Acquiring Firms**	**All Firms**
Number of Firms	3,938	3,291	1,121,748
Firm Size	44.13	173.00	6.87
Average Firm Age (year)	4.23	7.71	6.52
Number of Workers	173,796	569,343	3,604,399
Age	34.59	35.42	33.56
Share of Male	58.23	64.58	59.11
Weekly Wage (gross, euros 2003)	756.99	820.26	683.04
Occupation:			
Share of Blue Collars	66.37	62.36	63.16
Share of White Collars	28.41	33.18	29.92
Share of Managers	1.18	1.99	1.25
Share of Top Paid Females	27.60	17.01	15.04
Top Industries:			
Clothing Manufacturing	12.38	12.23	3.98
Footwear Manufacturing	5.27	5.53	2.78
Machinery Manufacturing	5.57	5.26	4.07
Construction	4.49	5.06	5.83
Wood Manufacturing	3.17	4.06	1.65
Clothing, Food and Retail	3.16	2.69	3.34

Notes: author's calculations on INPS data. Characteristics of acquired and acquiring firms are measured in the month before prior to the takeover event. Characteristics for the overall sample of firms are taken at the median date over the observation period.

Table 3.2: Female managers and acquiring firms' workforce composition

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Dep. Var.: % Female Managers	0.050***	0.049***	0.080***	0.080***	0.071***	0.072***
	(0.009)	(0.009)	(0.016)	(0.016)	(0.015)	(0.015)
Observations	143,440	143,440	143,440	143,440	143,440	143,440
Panel B						
Dep. Var.: % Female Blue Collars	0.034***	0.035***	-0.017	-0.016	-0.017	-0.015
	(0.007)	(0.007)	(0.013)	(0.013)	(0.011)	(0.011)
Observations	143,440	143,440	143,440	143,440	143,440	143,440
Panel C						
Dep. Var.: %Female White Collars	0.034***	0.035***	0.019	0.018	-0.009	-0.009
	(0.010)	(0.010)	(0.017)	(0.017)	(0.014)	(0.014)
Observations	143,440	143,440	143,440	143,440	143,440	143,440
Panel D						
Dep. Var.: % Managers	0.028***	0.028***	0.022**	0.022**	0.015**	0.015**
	(0.006)	(0.006)	(0.010)	(0.011)	(0.007)	(0.007)
Observations:	143,440	143,440	143,440	143,440	143,440	143,440
Panel E						
Dep. Var.: % Blue Collars	0.016***	0.016***	-0.008	-0.008	-0.031***	-0.031***
	(0.006)	(0.006)	(0.011)	(0.012)	(0.008)	(0.009)
Observations:	143,440	143,440	143,440	143,440	143,440	143,440
Panel F						
Dep. Var.: % White Collars	-0.029***	-0.027***	0.011	0.012	0.025**	0.026**
	(0.007)	(0.007)	(0.013)	(0.011)	(0.013)	(0.011)
Observations:	143,440	143,440	143,440	143,440	143,440	143,440
Controls:						
Date of Takeover	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Exposure Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Acquiring's Controls:						
Size Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Workforce Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry (3-digit)	Yes	Yes	Yes	Yes	Yes	Yes
Acquired's Controls:						
Size Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Workforce Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry (3-digit)	Yes	Yes	Yes	Yes	Yes	Yes
Acquired Controls* After	No	No	Yes	Yes	Yes	Yes
Acquiring Controls* After	No	No	No	No	Yes	Yes
Takeover Fixed Effect	No	Yes	No	Yes	No	Yes

Notes *: p-value<0.10, **: p-value<0.05, ***: p-value<0.01. Standard errors in parenthesis are clustered by takeover. Workforce characteristics for both the acquired and the acquiring firm are measured at the month prior to the acquisition event; these are: average wage, average age, share of females, share of blue and white collars, share of female blue and white collars. Exposure dummies are months from and to the takeover event.

Table 3.3: Female managers and acquiring firms' outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Dep. Var.: Log Acquiring Firm Size	-0.275***	-0.278***	-0.068	-0.073	-0.049	-0.050
	(0.033)	(0.034)	(0.060)	(0.061)	(0.046)	(0.048)
Observations	141,069	141,069	141,069	141,069	141,069	141,069
Panel B						
Dep. Var.: Acquiring Firm Survival	0.001	0.001	0.006	0.006	0.007	0.007
	(0.004)	(0.004)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	143,440	143,440	143,440	143,440	143,440	143,440
Panel C						
Dep. Var.: Variance Log Wages	-0.022***	-0.022***	-0.029***	-0.028***	-0.027***	-0.026***
	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)
Observations:	143,440	143,440	143,440	143,440	143,440	143,440
Controls:						
Date of Takeover	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Exposure Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Acquiring's Controls:						
Size Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Workforce Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry (3-digit)	Yes	Yes	Yes	Yes	Yes	Yes
Acquired's Controls:						
Size Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Workforce Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry (3-digit)	Yes	Yes	Yes	Yes	Yes	Yes
Acquired Controls* After	No	No	Yes	Yes	Yes	Yes
Acquiring Controls* After	No	No	No	No	Yes	Yes
Takeover Fixed Effect	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3.2.

TABLE 3.4: Female managers and incumbent workers' employment outcomes

	Probability of Job Retention		**Probability of Job Switch**		**Log Wages**	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
% Female Managers	-0.042	-0.041	0.027	0.026	0.001	0.004
	(0.048)	(0.041)	(0.047)	(0.038)	(0.013)	(0.017)
% Female Managers* Female		-0.002		0.001		-0.001
		(0.042)		(0.032)		(0.014)
Panel B: Individual fixed effects included						
% Female Managers	-0.042	-0.042	0.028	0.028	-0.003	-0.003
	(0.052)	(0.043)	(0.047)	(0.041)	(0.011)	(0.008)
% Female Managers* Female		-0.007		0.002		-0.005
		(0.045)		(0.037)		(0.011)
Controls:						
Date of Takeover	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Exposure Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Acquiring's Controls:						
Size Dummies	Yes	Yes	Yes	Yes	No	Yes
Workforce Characteristics	Yes	Yes	Yes	Yes	No	Yes
Industry (3-digit)	Yes	Yes	Yes	Yes	Yes	Yes
Acquired's Controls:						
Size Dummies	Yes	Yes	Yes	Yes	No	Yes
Workforce Characteristics	Yes	Yes	Yes	Yes	No	Yes
Industry (3-digit)	Yes	Yes	Yes	Yes	Yes	Yes
Acquired Controls* After	Yes	Yes	Yes	Yes	Yes	Yes
Acquiring Controls* After	Yes	Yes	Yes	Yes	Yes	Yes
Takeover Fixed Effect	No	Yes	No	Yes	No	Yes
Observations	20,052,240	20,052,240	20,052,240	20,052,240	17,914,660	17,914,660

Notes *: p-value<0.10, **: p-value<0.05, ***: p-value<0.01. Standard errors in parenthesis are clustered by takeover. Workforce characteristics for both the acquired and the acquiring firm are the same as the ones described in Table 3.2. Exposure dummies are months relative to the takeover event. Individual characteristics are: age dummies, gender and the occupation at the month before the acquisition event.

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